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EXPERIMENTS

ON

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MR. BABBAGE'S METHOD

OF

DISTINGUISHING LIGHT-HOUSES,

REPORTED TO A COMMITTEE OF THE

- Treas. Dept. -

UNITED STATES LIGHT-HOUSE BOARD,

BY

J. H. ALEXANDER.

WASHINGTON:
GOVERNMENT PRINTING OFFICE.
1861.

JUL
NEW-YORK.

REPORT.

To the Professors A. D. BACHE and JOS. HENRY,
Committee, etc., etc., etc., U. S. Light-house Board:

BALTIMORE, 26 *December*, 1854.

GENTLEMEN : I have the honor herein to make report of the experiments, with whose conduct you some time since were pleased to charge me, upon the Numerical System of Light-houses, which has been proposed by CHARLES BABBAGE, esq., and which aims at introducing, by means of appropriate periodical occultations, a more marked and better distinction among lights than the characteristics employed hitherto afford ; so as, among other things, to enable the mariner, without previous acquaintance, readily, accurately, and certainly, to identify any and each individual light visible on the coast or harbor to which he is approaching.

These experiments would have been made, and their progressive results, earlier, had I not expected, agreeably to information I received, that Mr. Babbage himself would have been present in the course of the past autumn, to institute, or at least direct them. So soon as it was ascertained that this expectation would not be realized, the subject was taken up, and all the necessary preliminary experiments performed, of which the following is intended to give account.

The documents transmitted to me for my instruction and guidance consisted of, 1st, Notes respecting Light-houses, by Mr. Babbage, printed on p. 156, *et seq.* of the Report of Officers constituting the Light-house Board, &c., Washington, 1852; 2d, MS. copies of two communications from the same gentleman to Professor Bache, under date of 28 August, 1853, and 21 January, 1854, respectively ; and, 3d, MS. copy of a Statement by the Commissioners of Northern Light-houses, (of Great Britain,) with reference to the plan proposed by Mr. Babbage for distinguishing lights. All these documents have been read and considered.

The first and second contain theoretical and practical suggestions of the inventor upon the merits and mode of establishing his system. The last sets forth general critical objections to it, and the mixed reasons which governed the decision of the Scottish Commissioners in declining to permit its adoption.

The aspects presented in this last document were, of course, not intended to be referred to me otherwise than as incidental to the general topic, as it had come before the U. S. Light-house Board. I might, indeed, have understood them as having been adjudicated and disaffirmed in the very act of committing to me the specialty of instituting experiments which would have been superfluous had the American Light-house Board accepted the broad grounds of the Northern Lights' Commissioners. There would, therefore, be no propriety in my referring to, even, much less discussing, any of the points in those grounds, further than as they touch and affect the physical theory of Mr. Babbage's plan, and of the experiments suitable to its illustration and perfection.

One of these points, to be sure, not thus physically connected, I had begun, upon your first reference to me and in advance of acquaintance with the document in question, to inquire into, with the view of ascertaining statistically the actual efficiency of the present system of distinction, and so the extent of desirable or necessary improvement. I had been accustomed to regard,—perhaps without sufficient knowledge, not being a mariner myself, and my opportunities for observation having been chiefly confined to occasions, at one time not infrequent, along the Chesapeake bay and the ocean shore adjacent,—such extent for improvement as quite considerable; and I supposed that a statement which would give the whole distance of coast lighted and accessible to vessels, and the whole number—1st, of lights existing along that coast; 2d, of vessels visiting it in any given period—say twelve months; 3d, of wrecks occurring during said period; 4th, of wrecks occurring either when or because the adjacent light-houses were not visible; and, 5th, of wrecks occurring either when or because the said lights, or any of them, were mistaken or could not be identified,—would afford data proper for a precise and reliable determination of the question. But to obtain conclusive materials for such a statement, I found beyond my means; and the only resort which I had for the last items, viz: the register of claims for losses against marine insurance in this city and in New York, soon showed that from these at least was to be expected no fruitful result.

It seems plain, however, that the partial statistics, though doubtless accurate as far as they go, of the coasts of Scotland, do not exhaust the condition of the question; and that the ratio of the whole number of night shipwrecks to the cases among them where the lights were seen but not recognised, is only one of the elements for its solution. Thus, it is manifest that this ratio must be, among other things, inversely as the whole number of lights on the coast; *i. e.*, that it must increase just as the whole number of lights diminishes. For instance, if we suppose, for the sake of illustration, that the number of lights on a given coast is diminished to unity, there could be no question as to the identity of this single light whenever it was visible, and thus all the shipwrecks occuring on that coast, which probably under such circumstances would be very numerous, might be quoted, according to the view of the document referred to, to sustain formal conclusions like its own, although here completely erroneous. In the same manner, this ratio might be shown to be materially influenced by other particulars. But this point need not be dwelt upon further.

The next one is more aptly connected here, viz: the loss of illuminating power accruing upon Mr. Babbage's plan, by all the difference in brilliancy between revolving and fixed lights. There is no doubt that such a difference does in fact exist; that it is unfavorable to fixed lights; and that Mr. Babbage, in his printed document, has prescribed these last as essential to the adoption of his plan. It becomes, therefore, of interest to define what is the amount of this difference in general; and also to ascertain how far the actual or possible mechanical arrangements of the proposed plan tend to increase or diminish it.

If the difference between the two classes of lights were so great as to render it expedient, irrespective of Mr. Babbage's plan, to disuse fixed lights altogether, the question would, of course, be either settled summarily, or at least restricted to the determination of features in the new plan increasing the efficiency of said fixed lights. But in point of fact, the supposition does not hold, since so far from fixed lights being inefficient or disused, a large proportion (nearly one half, I suppose,) of the great sea lights on our coast, at least, is of this character. There can logically, then, be no absolute exclusion of Mr. Babbage's plan on this account, and no argument against it at all, in all those tolerably numerous cases where fixed lights are at present actually employed. Such an argument could only apply to the cases of converting existing revolving lights into fixed ones.

The amount of brilliancy lost by such conversion has been ordinarily and in professed treatises stated but vaguely. Thus, Mr. Alan Stevenson, on this point, merely says, that "it seems to be a natural consequence of the physical distribution of light that fixed lights which illuminate the whole horizon should be less powerful than revolving lights which have their effect concentrated within narrow sectors of the horizon." And the document of the Scottish Commissioners, already referred to more than once, uses terms not less general, though more emphatic, when it says, that "the conversion of revolving lights to fixed lights would prodigiously deteriorate their power;" or again, when it suggests that "the enormous difference of power between a fixed and a revolving light must be obvious to any one who is acquainted with the optical principles on which light-house apparatus is constructed."

The actual ratio of brilliancy between the two classes admits, to be sure, of being calculated only for separate cases where the elements of the apparatus are given, and its range or the distance of the observer is assumed. This last item affects, of course, materially fixed lights, the mass of whose rays more divergent is subject to the ordinary law of intensity diminished in proportion to the squares of the distance at which they may be observed, while the brilliancy of the more parallel beams of the revolving lens or mirror is, theoretically at least, independent of distance. But these terms must be taken altogether as comparative; for while, in the fixed apparatus, the diverging rays of the unassisted lamp-flame are so far paralyzed as to be augmented in intensity several hundred fold, in the revolving apparatus provision is made for an amount of remaining divergency, without which a parallel pencil could produce but the briefest flash, perhaps imperceptible, and, at most, hardly useful.

The trouble of calculations, however, may be dispensed with for our present aim, by resort to experiments, which give, with as much precision as belongs to photometric methods, or as is required for practical purposes, ratios of brilliancy that may be considered constant. Such experiments have been reported by Mr. Leonor Fresnel upon annular lenticular apparatus, as follows :

	FIRST ORDER.		SECOND ORDER.	
	Unity Burners.	Proportion.	Unity Burners.	Proportion.
Fixed light, constant brilliancy,	560	1	264	1
Revolving light, maximum "	4,400	8 n'rly.	1,184	4.5 n'rly.

It is obvious that we need not compare lower orders; both, because, where they are suitable, there is no question of the sufficiency of fixed lights and because the experiments show that as the size of apparatus diminishes, the lustre, whether in motion or at rest, tends to approach equality.

Such, then, are the relations of absolute brilliancy. But this is not the only relation that should be taken in account, for it will be observed that while the brilliancy of the fixed light, such as it is, is constant and uniform in every azimuth around the whole horizon, (except only those small spaces where the vertical opaque joints of the lenses and framework are interposed,) that of the revolving light is but momentary in a given azimuth of extreme narrowness, (being seven times as bright as a fixed light on an arc of less than 1,000 feet at a distance of ten miles,) which it leaves again in darkness for a shorter or longer period according to the rate of revolution of the apparatus. The effect, therefore, of this last class can be useful in the photometric ratio of its brilliancy only in that particular azimuth; and hence, to establish and compare the absolute useful effects in navigation and to vessels that may be assumed to need the light at the same instant in different azimuths, it is necessary that the numbers representing the brilliancies be combined with the lengths of the circular arcs respectively subtended.

When this is done, throwing out all accessory parts common to both classes, we have the following proportions :

	FIRST ORDER.	SECOND ORDER.
Fixed light, useful effect....................	1.	1.
Revolving light, "	1.0368	0.9484

In which the calculation agrees with observation, that as the remoteness of range becomes of less moment the advantage of fixed lights increases.

Consistently, then, with these statements, both of useful effect and of absolute brilliancy, it may be concluded that wherever the offing, requisite for the warning of the mariner and the safety of the vessel, does not exceed the range of a fixed apparatus, there is no reason, except for distinction's sake, to erect any other.

This condition, I am informed, exists extensively along our north-eastern coast, where the task of the navigator at present is to distinguish, at distances fully penetrable by a fixed light, among various inlets, often not very remote, what is his proper course. Here, then, for instance, and elsewhere, where like conditions are found, the plan of Mr. Babbage is fully admissible.

It should be, and is, among the experiments you have authorized me to institute, to observe how far the alternation of light and darkness in the occultations, upon Mr. Babbage's plan, tends to increase or diminish the efficiency of a given light. That the space-penetrating power of the rays themselves should be augmented, is not probable ; but that, within the limit of penetration otherwise, the sensibility of the retina of any observer should become more acute by the alternations of these opposite states, is quite likely. Such was, in fact, the case with myself in some observations I have already made, but the range used was so short as not to allow any positive, universal deduction to be predicated.

In order to try this point most suitably and conclusively there ought to be a second light, equivalent with the first, placed alongside, so as to afford continually a standard for comparison. But as to establish a double arrangement of this kind, if possible at all, would be, I presume, very inconvenient, I limit myself to its mere mention, anticipating a reasonably reliable result from alternating observations of the same light, now left to burn steadily, now undergoing successive occultations.

Finally, in connection with this whole question of difference in lustre between fixed and revolving lights, it is to be observed that though Mr. Babbage's printed notes referred to the use of the former, yet his manuscript communications subsequently indicate how the distinctions of a numerical system may be attained upon the latter also. But as these modifications appear to me of secondary importance, I have thus far postponed their study, and am unprepared to express any opinion.

After these preliminaries, which I begin to fear you may find more than sufficiently tedious and out of the record, I can now pass to the proper subject of this letter, namely, the method and results of the observations that have been made and of the experiments that have been tried.

The illuminating apparatus used was a third order, large size, annular Fresnel lens, with a cupola, now somewhat ancient, but in tolerable repair. It was fitted with three revolving external lenses for eclipses; of which, however, I made no use further than sometimes to fix them in the direction of the observing station, in order, by the increased intensity of the light thus afforded, to have a virtual effect of variation in the range.

The place for its erection I owe to the courtesy of the managers of the Maryland Institute in this city, who allowed the large south chamber in the third story of their hall to be used for its accommodation. As the lamps which accompanied the lens did not work very well, and for other reasons of cleanliness and convenience, I preferred for its illumination to use gas, with a suitable burner that preserved the normal volume of flame corresponding to the capacity of the lens. I made no special observations upon the different intensities of light from the oil lamps and from the gas burner, as not affecting practically the subject. In general, however, I may say that the gas light was the more brilliant of the two.

The place from which it was intended to be observed was Fort McHenry, lying distant about two and a quarter miles across the river. After some trouble, however, it was found that this could not be occupied, the lens having become hidden, I believe, by some buildings erected since it was put up first. The station actually used was the Merchants' Observatory on Federal Hill, distant less than a mile, but with a wide extent of the harbor intervening. This water surface and the vapor rising from it and from various steamboats, together with the smoke of the city itself which descends upon the harbor, produced upon the light itself, as I had occasion to notice repeatedly, an effect corresponding to a much augmented distance in a purer atmosphere.

The mechanism for the occultations was arranged to be worked by hand, in order to leave the utmost scope in these preliminary experiments for the variety of combinations that might be demanded. It consisted of a cylindrical chimney of sheet iron, with a collar of brass below, fitting, when down, to a corresponding balcony on the burner, worked through a vertical lift of five inches along steel guides by a lever forked at one end to embrace the diameter of the chimney, and flattened at the other into a convenient finger-key, while its fulcrum was on an upright shaft hinged to the general base of the mechanism, and, by its rocking motion, preserving the parallelism and verticality of the cylinder. A metronome, adjusted to beat seconds, furnished the means of counting time, and two assistants were readily trained—the one to work the apparatus, the other to count the intervals and check off the programme of the various operations for each night's work.

After this had been arranged, it was my habit to resort to the Observatory station with another assistant, who either announced the occultations while I observed the time with a seconds' watch, having but one metronome, or counted the time while I noted the occultations. But I soon was able, after a little familiarity, to dispense, except occasionally, with any reference to the watch, (especially in the last systematic series,) it being easy to count time in the head with an accuracy sufficient to prevent any mistake or confusion.

When the mechanism was ready for work, the first point to be determined was the character of the occultations and reappearances, whether they should be sudden or gradual. And this was tried in all phases, by—1st. Lifting the cylinder suddenly, and, at the end of the given period, letting it drop suddenly; 2d. Lifting suddenly, and, at the proper period, letting it descend gradually; 3d. Lifting gradually and then allowing a gradual descent; and, 4th. Lifting gradually and dropping suddenly. Of these, the most characteristic in its effect upon the retina was the last. But in the actual experiments subsequent, partly on account of the difficulty of producing by hand a uniformly gradual elevation, and partly because I found the same phenomenon imitated in some sort by casual vails of vapor or smoke that swept between the lens and me, the first phase was alone employed, which gave besides a sharper and more certain definition of the periods.

The next thing to be settled was the duration of the occultations. A period of one second I found too short to obliterate the impression of the previous lustre; a period of two seconds was more satisfactory in this respect, but it was sometimes embarrassed and confused by the nearly similar duration of those obscuring currents of which I have already spoken; a period of five seconds, which was then tried, was obviously in all respects unnecessarily protracted, and I finally settled down on one of three seconds as affording the maximum of advantage, at least under the local circumstances of the experiments, and, I incline to think, universally.

These points having been established, there were manifestly two systems under which they might be applied and the object attained—1st. By causing occultations, of constant duration, to alternate with reappearances whose repetition should indicate the number of digits intended to be expressed, while their varying periods of duration should be characteristic of the numerical place of the digits, respectively; or, 2d. By causing occultations, of constant duration, to occur as many times during a given period as there are digits to be expressed. Either of these contains, I apprehend, the substance of Mr. Babbage's plan.

In the second system which happened to be tried first, (and the most simple to work,) it is obvious that the period during which the occultations occuring are to be counted, has a practical limit, and that, in general, it should be as short as possible. The shorter this period, the more expeditious is the determination of the number of digits expressed, *i. e.*, of the number of the light-house in question. Further, it is to be considered that one count of occultations, during an assigned period, might not be always satisfactory enough to be relied on, and therefore it would be fair to allow twice the assigned period for the ascertainment of the number in question.

Again, on the other hand, the shorter the period assigned, the less with occultations of constant duration will be the number of digits that can be expressed; and, in so far, the scope and fecundity of the system itself become limited.

On consideration of all these points, and of some others which do not require mention, I took, as a convenient period, one minute, which answered very well, provisionally, for these experiments, and, I suppose, would be suitable universally. I took, also, finally, for the duration of occultations, three seconds, as had been shown before to be most advantageous, but only after having repeated and compared other durations up to five seconds in experiments upon this very system.

Of course, with these definitions, the expression of the digits or the numerical value of the phenomena will be—

For *one* digit, or light-house No. 1, one occultation per minute;
For *two* digits, do. No. 2, two do. do.
For *three* digits, do. No. 3, three do. do.

and so on.

These may be arranged with greater clearness in the following table:

No. of Light-house.	Period of— Illumination.	Occultation.
1	57 sec.	3 sec.
2	27 "	3 "
3	17 "	3 "
4	12 "	3 "
5	9 "	3 "
6	7 "	3 "
7	$5\frac{4}{7}$ (6)	3 "
8	$4\frac{1}{2}$ (5)	3 "
9	$3\frac{2}{3}$ (4)	3 "
10	3	3 "
etc.	etc.	etc.

Beyond the No. 10, however, where the illumination and occultation become of equal duration, it would not be suitable to go with the assumed period of one minute; which period, considering all its practical advantages, it would be hardly advisable to increase.

The application, then, of this system will be to arrange the lights in groups of ten; the distance between any number in one group and the same number in the adjoining groups being supposed great enough to prevent any possibility of mistake. Such distance would probably be about 200 miles, a space beyond any assumable error in reckoning.

Of course, the individuals in any group would not be arranged in numerical order, but in such sequence as would allow of the utmost difference between each. For instance, I arranged them, in repeating the experiments, in the following succession:

First Group.	Second Group.
1 4 7 10 2 5 8 3 6 9	1 4 7 10, etc.

Here a mistake of at least three occultations would have to be made in the count in order to produce confusion between any adjacent lights. A mistake of a single occultation corresponds to a space of 60 miles in reckoning. I can affirm from experience,—in the comparative quiet and safety of a station on land, it is true,—that a single observer need not be in error to amount of the greater of these mistakes in any case ; and that it would be difficult for two observers, one watching the light and the other counting time, to be as much out as the least of them. What allowance in this respect should be made for storm and darkness on ship-board, I have no means of estimating.

It must be admitted, however, that this system has the disadvantage (as the document of the Scottish Commissioners has in substance indicated) of requiring the simultaneous observation of two phenomena, viz : the disappearance of the light, and the movement of the index of a time-keeper ; and although, with suitable provisions, this double observation need not be a source of uncertainty, it still remains in its nature an element of embarrassment. Withal, this system does not materially increase the scope of distinctiveness as possessed already by existing ones ; although the individual distinctions it affords are, in my judgment, more characteristic and reliable. I therefore willingly turned the experiments to the other system first indicated, as the most appropriate in theory and most fecund in application.

This, which, as already said, consists in the alternation of constant occultations, with reappearances of varying duration, demands, when those variations of duration are well characterized, no reference to a time-keeper on the part of the observer, and reduces his task to the phenomena of the light only. Here he has, to be sure, two things to notice, viz : 1st, the characteristic durations of the illuminations, and 2d, the number of repetitions of each, or, what is the same, the number of occultations under each character. But in regard to the first, the distinctions are susceptible of being made so emphatic as not to call for any special exercise of the faculty of comparison ; and the second, which is virtually the sole item, is so simple as to require in no case any other mechanical adjunct than the fingers.

Of course, in this system, as in the other, there is a practical limit to the durations that can be adopted ; and more than in the other will there be occasions for a second observation, or a portion of a second observation, to supplement or confirm the first. This will be more apparent hereafter.

It is not necessary to detail the trials I made of various characteristic periods, in order to see which would combine the greatest advantage in all regards. It is enough to say that, after several such trials, I came to settle down on the following, viz: a constant duration of 3 seconds for the occultations; a period of 3 seconds illumination for units, of 10 seconds for tens, and of 20 seconds for hundreds. Further, I arranged that these should read, as it were, from left to right—*i. e.*, that the count should begin in the mechanism with the digit of the highest place. And I did not find it necessary to interpose any pause to indicate the completion of the count of the proper number, but allowed the mechanism, after the occultation which followed the last repetition of units, to recur at once to the repetition of hundreds, if there were hundreds, or of tens, if the number of digits did not exceed that rank.

For example, to express the number 345,—the highest, I believe, that is likely to occur on our coast,—the mechanism, supposed to start from a state of rest, *i. e.*, from an occultation, will show the light for 20 seconds, followed by an occultation of 3 seconds, *three* times; then for 10 seconds, followed by the constant occultation, *four* times; then for 3 seconds, followed by the constant occultation, *five* times; then for 20 seconds, followed, etc., as before; and so on, without intermission.

Perhaps this will be more plain, by exhibiting the actual programme furnished to the assistants at the lens, so far as covers this single number, thus:

Programme, etc., for No. 345.

	CYLINDER.			
	Up.	Down.	Times.	
Signal to prepare:	2 *sec.*	2 *sec.*	20	
3, hund.	20 "	3 "	1	Repeat 10 times.
	20 "	3 "	1	
	20 "	3 "	1	
4, ten	10 "	3 "	1	
	10 "	3 "	1	
	10 "	3 "	1	
	10 "	3 "	1	
5, unit	3 "	3 "	1	
	3 "	3 "	1	
	3 "	3 "	1	
	3 "	3 "	1	
	3 "	3 "	1	
Signal to prepare:	2 *sec.*	2 *sec.*	20	etc.

These *signals to prepare* were merely arrangements for convenience between the assistants at the lens and myself, to indicate the passage to the execution of a new number. Further, I had also a pre-concerted set of fire-signals at my own station, watched for by a third assistant, of whom I did not speak before, at the lens, by means of which I was able to direct the suspension of any particular number during its execution, or its repetition to a greater number of times than prescribed in the programme, or the passage to another number at discretion, &c., &c.; and also to make other communications that might occasionally become necessary.

I found no difficulty at any time, by a slowly-uttered count, *one, two, three, etc.*, (independent of the second's watch, and capable of being originated by any one, who has a notion of time in his brain, from the beats of his pulse,) in identifying beyond the risk of mistake any of the numerical ranks, and thus readily distinguishing between the hundreds, tens and units; and after a while I found that even such oral count was dispensable, and that the difference of duration fully characterized the rank of the numbers that were being executed.

My assistant at the station, who had not previously been accustomed to such observations, speedily became as expert as myself; and although generally left purposely in ignorance of the number that was intended to be executed, did not fail to announce it correctly after two counts, and, ordinarily, after one.

It will be at once seen that it is no matter at what part of the revolution of the mechanism the count is begun to be kept, the characteristic durations unfailingly point out the order of numerals which are being executed. Thus, supposing, in the example of the schedule just given, that the observer first begins to notice in the middle of a revolution, (which will be the most disadvantageous point,) *i. e.*, when the mechanism is executing the first of the tens; he will at once recognise the characteristic of rank, either instinctively or by an oral count; and he will go on to count three tens, when an emphatic change in the durations will show him that he must reckon units, of which he will then count five; when he will perceive a still more emphatic characteristic introduced which will assure him of the termination of the number, (*i. e.*, of its last figure,) and also that the proper number is among hundreds. When he has counted through the hundreds and through the tens, he need not, except for confirmation, count any further, for he will have the number in full.

If, further, a similar disposition be made of the lights in this as was spoken of for the other system, viz: out of the regular numerical sequence, whereby numbers that differ but slightly would occur at remote intervals, the proper number will be ascertainable inferentially, even before the full count is completed.

Of course, to make one such full count with high numbers consumes time. Thus, to count the number 299, which is the longest up to the limit before supposed likely to exist on our coast, requires, assuming the count to be commenced in the middle of a revolution, (the least advantageous point,) *four minutes thirty-two seconds.* To count this number twice, consecutively, would require *eight minutes nine seconds.* I am unable to speak, of my own knowledge, as to whether periods like these would be, upon critical occasions, admissible or not. The former, at least, I should suppose, would not be, in general, too long.

But if it should be judged to be so in particular localities, a remedy might be had by disposing the low numbers at all those points where the offing is precarious, and where the mariner cannot afford to be so long as five minutes without an identification of the light. The high numbers would then occur in those situations where a few minutes, more or less, would not be of disastrous consequence.

To look at this topic of time in another aspect, perhaps more curious than useful, it may be observed that the *mean* time of counting all the available numbers of the whole series, extending to 350, is less than *two minutes*, or, more nearly, 113 seconds.

The term *available numbers*, is used here because there are, as will readily suggest itself upon reflection, some numbers in the series that cannot be expressed without having an additional signal to signify *zero;* and some, also, as the entire unit series from 2 upwards, that cannot be executed without a still additional signal to indicate a stop. It seemed to me better to surrender the numbers of this sort than to cumber the system with multiplied signals.

In point of fact, it is not all the numbers containing or terminating with zero that need be excluded, but only those where there is but one significant figure and that one greater than unity. Thus, the number 10 is expressible, and also 100, by their appropriate characteristics; the former after ten repetitions, and the last after passing three (under the preceding assumption of the whole number of lights being less than 400) being recognisable as unitary. But the numbers 20, 30, 40, etc., and 200, 300, etc., could not be recognised. Where the significant figures are in two places, the want of a signal for zero produces no confusion. Thus, 102 and 120 can be readily identified, and so of others.

It will be found, then, in this way, that only eighteen numbers have to be surrendered out of the whole three hundred and forty-nine, and that the abandonment after all is in reality of no moment, since the same discount holds good up to 400; and the system itself with its present equivalents and the loss of but six more unavailable numbers goes up to 999—a large fraction of all the light-houses on the globe.

I consider, then, in view of all that has been said, that this system may be advantageously applied at any and all points where the range demanded does not transcend the power of a first order Fresnel lens; and as I am not informed of any existing requirement greater than this, I take it further as applicable without reserve along our whole coast.

In order, however, that it may have the advantage of criticism by other observers, and an exemplification (as I anticipate) of its utility, I beg to suggest that I be allowed to erect temporarily the present lens, or some other with appropriate self-acting machinery, in some actual light-house. If this suggestion meets your approval, the Bodkin light at the mouth of the Patapsco will be a convenient place, being easily visible from the city, and at a distance about 15 miles across land and water, adequate to developing all the good and bad points of the system as cannot be predicated of the limited range I have been able to use hitherto.

I have not yet had the opportunity of ascertaining how far the lantern at the Bodkin is capable of accommodating the apparatus; but presume that it is sufficient.

Of course some time must elapse, after the authorization asked for here shall have been obtained from the Light-house Board, before the apparatus can be erected; not so much on account of the preparations necessary as for the due publication of notice to mariners which should precede any change in an established light. I am uninformed as to the length of time allowed for this last object, but can undertake to have all the mechanical arrangements in readiness for any given day after 31st January next. To transport and erect the apparatus will require one day.

In respect to the mechanism necessary for working the occulting cylinder, which is already in great part designed, I have not thought necessary to delay this report until I had prepared and could copy all the drawings for it, because it is extremely simple. The descent of a weight, requiring to be wound up only once during a long winter night, furnishes the maintaining power which is communicated through a short train and regulated by a short counter weighted seconds' pendulum, which, considering the avoidance of repairs, I prefer to a fly governor. On the arbor of the swing-wheel can be adapted at pleasure any of the cams working the lever-lift for all numbers which can be executed in one minute or less, which I take to be enough for the present purpose, or this adaptability may be extended to those numbers even which require two minutes and under. In actual practice, when each individual number will have its own separate machine, it will be better to gear the cam-shaft to the wheel-work elsewhere than at the scapement.

The necessity for machinery at all, and the inconvenience of readily replacing it with a duplicate, constitute with me, in the abstract, the most serious objection to the new system; as, however, this is an objection that applies equally to all revolving machinery and to the lights of that class, and also in a measure to the clock-work for the oil pumps, and as better than in either of these cases, the occultation machinery can be provided for by hand through an entire night if need be, the new plan stands in fact upon an equal footing with existing ones. The nature of the work to be done, besides, allows and even requires that the machinery be very strong and substantial, and in so far the risk of accidental disorder becomes less; nor would this risk be increased by gearing it on (as will be very easy) to do also the work of the lamp pumps. By this modification the construction of the lamps themselves will be cheapened and improved, and the aggregate machinery of a light-house would be lessened in quantity, in cost, and in liability to disaster.

In relation to the other uses of which Mr. Babbage's plan is capable, such as telegraphic signals along shore, to and from vessels lying-to or in distress, &c., &c., I have considered it more likely that a full and just conclusion would be arrived at ultimately in the premises by confining the present inquiry to its main and more immediate feature of utility, than if it were complicated by the consideration of various incidental applications, in the adoption of which regard has to be paid to more than mere mechanical arrangements or adaptability. And I have accordingly so confined it, leaving those incidentals, though seen clearly to be of great philosophical interest and public convenience, to their own gradual development, or at least to future investigation.

In the hope that the several considerations that have been presented will appear to you satisfactory, and that the conclusions arrived at will meet your approval, and awaiting your further directions in the premises, I have the honor to remain, gentlemen,

J. H. ALEXANDER.

FIRST REPORT

ON

OILS:

ADDRESSED TO

THORNTON A. JENKINS, ESQ., U. S. N.,
U. S. LIGHT-HOUSE BOARD, ETC.,

BY

J. H. ALEXANDER.

THORNTON A. JENKINS, *Esquire, U. S. N.*,
Secretary, etc., etc., etc., U. S. Light-house Board,
Washington, D. C.

BALTIMORE, *June* 26, 1855.

SIR: I have the honor herewith to enclose the report of Professor MORFIT, of the University of Maryland, and myself upon certain samples of oil which you caused to be forwarded to me in January last, for examination and analysis, and which have been attended to with all the expedition compatible with the extent and accuracy of the research.

The aim, as I considered, should be not only to determine the existence of adulterating ingredients with sperm oil in two of said samples, and, consequently, the factitiousness of those samples, and also the character and degree of adulteration so as to be able to assign, with a certain precision, the respective proportion of each; but also able to devise some method of easy application by which these points could be hereafter ascertained with tolerable approximation in the practical inspection of oil which the light-house service is understood to require.

This extent of aim, of course, extended also, varied and multiplied the processes that would have been ordinarily requisite in a less conclusive and more limited examination. I hope that the object I thus held up to myself is one that you likewise desired and approve; and that the discretionary labor bestowed in its attainment has been discreetly bestowed and will not be regarded as incommensurate or inconsistent with the general plans of the Board, of which I can but imperfectly judge.

The research, in respect to the two first named points, will be found, I think, to have been attended with reasonable success; and as among the evidences of this, I have caused to be put up and transmitted to you, a number of bottles, two of which contain some of the H. oils, respectively, and the rest hold certain artificial oils, which all correspond more or less with the factitious samples of H., while a few of them represent and are to be considered as reproducing with physical identity those samples, respectively. These bottles are all lettered according to the numerical sequence of the mixtures mentioned on page 6 of the report, and can be in this way identified.

Now, if the proprietor of the H. oils can constantly distinguish his own oil among these samples, (except possibly by the smell of one of them,) he will be in possession of a faculty or a test (as the case may be) not ordinarily exercised by or known to chemists. And even then, a capital difficulty will remain, viz: to show that several of the artificial oils are practically different from and inferior to his own.

In the nature of the case, this is the best proof available for showing that the problem you have submitted to me, has been solved. For, as you will perceive when you peruse the report following, hardly any of the tests applicable are absolute in theory; while even these are liable in practice to be restricted and masked by the slight irregularities and variations in composition and externals so frequently found in different samples of the same organic substance. Therefore it is that in such cases as in the present, we are forced to resort to comparative methods which act, as it were, by exhaustion, and to rely on (so to speak) circumstantial evidence

In regard to the last-named point of research, viz: the device of some method of inspection by which the identity or difference between given samples of oil and a standard specimen can be promptly and reliably ascertained, the following conclusions may be considered as settled:

1st. That there is a certain rise of temperature produced by the addition of a half-volume of sulphuric acid of commercial density to any given volume of either of the three natural oils—sperm, whale, and lard—herein considered, appropriate to the said oils respectively and constant for the same oil within a degree or two of Fahrenheit's thermometer. To this rise of temperature under such circumstances I have given the name *epitherm*, and so it may be said, in brief, that each of these oils unadulterated (and probably all others) has its own characteristic epitherm.

2d. That this epitherm affords a reliable means of distinguishing, among these, several oils in their natural state.

3d. That any mixture of any two of these oils has a characteristic epitherm which may be equated arithmetically within two or three degrees of Fahrenheit's thermometer, upon the proportionate quantities and epitherms existing before and at the time of mixture.

4th. That this epitherm also, taken with specific gravities, affords a reliable distinction between the said natural oils and any factitious mixture of them.

5th. That the epitherm, taken with specific gravity, affords a probable means of determining even the proportions of natural oils existing in the mixture within the limit (say 3° F.) of the errors of observation.

This last item is not so material in the practical inspection for light-house purposes, where, I apprehend, the object is to ascertain the identity of delivered samples with the sample contracted for, and still less material, so long as the contracts are made for pure sperm oil, whose epitherm is so far below that of any probable (I might almost say possible) mixture as even to render the observation of specific gravity unnecessary. If, for example, the rise of temperature does not transcend 72°.5 F., it may be safely said that the oil in question is an unadulterated sperm oil. If the light-house service should hereafter admit other than unadulterated sperm oil into its contracts, having an epitherm higher than this, the coincidence of epitherm and specific gravity respectively in the article contracted for and that delivered will render the identity of the two in the highest degree probable, or, as we say ordinarily, morally certain.

In view of the value of this index of identification, I respectfully suggest that you should consider the propriety of its being referred to in future contracts as an element to be observed and insisted on.

Should your consideration prove favorable, or should you, on the other hand, prefer the employment of the index in question, without announcing it, and so setting ingenious persons to work upon contrivances for evading it, I can very promptly describe, or, if you please, furnish the requisite apparatus, which consists only of a thermometer and a glass or porcelain beaker. It is necessary that these beakers should be, as nearly as possible, uniform in material, capacity, and mass, so that their absorption and radiation of heat shall be respectively the same.

These precautions would be of more and more importance if the method were to be applied (as it will without doubt come to be) in laboratories, inversely, as a means of analysis ; but in its direct application for determining identity merely, they do not seem necessary to be dwelt on here any more than a good many other particulars which are of extreme theoretical interest, and may hereafter be of practical moment.

As under this aspect, for instance, I pass by any further details upon the new chromatic test for lard oil, mentioned in the report, whose utility in defining ternary mixtures, when such cases have been detected by the discordance of epitherm and specific gravity, and even in detecting such in advance, will, I anticipate, be hereafter recognised. As a means of detection and determination in the simply adulterated summer sperms, wherein lard, I am informed, enters largely as an ingredient, its direct application would be of immediate value.

At a future and more favorable time, I think it would be worth while to take up the special subject of inspection of oil for the Light-house service, and make it the topic of extended consideration and report.

In the mean time, with the hope that what has been said and done so far will meet your approval and be to the advantage of the Light-house service,

I remain, sir, with great respect, yours,

J. H. ALEXANDER.

REPORT ON OILS.

The oils forming the main subject of this report were standard samples of winter and summer strained sperm oils and whale oil, furnished by Mr. E. M. Robinson, of New Bedford; a sample of lard oil, furnished by Messrs. George & Jenkins, of Baltimore; and two samples of oils furnished by Mr.——— ——— of—————, both marked with his surname, and one bearing an additional indication of 40°, presumed to be of the temperature at which it was expected to remain fluid. All these samples, except the lard oil, the first specimen of which was in a glass pint-bottle, came to hand in well-secured tin cans, and appeared to be in good condition. The observations and experiments to which they have been subjected, and the results of their examination, are presented here briefly and orderly as follows:

PHYSICAL PROPERTIES OR EXTERNAL CHARACTERS.

1. *Color.*—This characteristic is only given here as ordinarily and formally necessary in any description of an article submitted to physical examination, but not as a conclusive or reliable test, for which it is both in the present samples too slightly marked, and in general too variable among different samples of the same kind. In point of

fact, the color of all these samples (except of the lard oil, which is light-golden,) is a sherry-brown, and the hue of each goes on deepening successively, though not by equal gradations, in the following order, viz: lard oil; H.; H.$_{40}$; sperm$_{34}$; whale; sperm$_{40}$. And the summer-strained sperm$_{40}$, which in constitution and source may be presumed to be the nearest to the winter-strained sperm$_{34}$, differs more from the whale oil in tint than does this last from the winter sperm oil.

2. *Odor.*—This characteristic, with perfectly natural oils, *i.e.*, those which are quite unadulterated, is much more reliable than that of color, but even more difficult to be defined in words. The odors of the lard and of the whale, especially, are quite distinct; while that of the sperm is sufficiently marked to enable one, after some experience, to distinguish with tolerable certainty, by the faculty of smell alone, among the three; but in the case of factitious oils—mixtures, for instance, of the three that have been mentioned—this test becomes less permanent and decisive; for it is not difficult to adulterate, aromatically, the mixture so as, if not entirely to mask for some time what ought to be the dominant odor, at least to confuse the several ones to such a degree as to render their separate appreciation the work only of singular acuteness or of good fortune.

In the present samples the odor of the lard, whale, and the two sperms, appears of natural distinctness and intensity; that of the H. oil pleasantly differs from all the four, leading to the supposition, in the absence of all other test, that some disguising aromatic had been added; while in the H.$_{40}$ the odor of lard leaves itself yet to be strongly suspected.

3. *Density.*—This is among the most precise and persisting characteristics of all oils, natural and factitious; and it was therefore ascertained with great care for the present samples, by counterpoising successively in each, in distilled water and in air at the same temperature, a ball of about an ounce troy weight, suspended on a delicate balance beam by platinum wire of about the $\frac{1}{500}$ of an inch diameter. Of course the proper correction was made, at the rate of 0.047 grains per inch, for the varying immersion to the nearest tenth of an inch of the wire, according to the accidental difference of level of the water and of the oils, respectively, in the beakers containing them.

The results thus obtained, at a constant temperature throughout, both for the oils and for the water, of 64°.5 Fahrenheit, were as fol

Water	1.—
Sperm$_{34}$	0.87971
Sperm$_{40}$	0.88056
H.	0.91005
H.$_{40}$	0.91184
Lard	0.91546
Whale	0.92000

This temperature of 64°.5 F. is so near the English standard of 62° F. that it becomes nearly as useful, and more safe, to give results actually observed. rather than run the risk, by any more systematic statement, of the errors which are more or less apt to attend the introduction of constant factors for reduction.

It is seen, then, that differences of density are plainly marked among the natural oils; and that, among the present samples, it is an unmistakable test. It became of interest, then, to ascertain how far it would be permanent and reliable for mixed articles, *i. e.*, whether in the alligation of the mixtures there was any change of volume occurring, which would complicate or embarrass the calculation of the proper densities of such mixtures.

For this were measured, with as much care as possible, equal volumes, at the same temperature, of lard, whale, and winter sperm oils, which were then severally well mixed and the specific gravities of the mixture taken as before; only in this case the temperature at the time of observation happened to be 65°.25 Fahrenheit. The following were the results observed and compared, with the specific gravities calculated from the densities of the original oils, respectively, without change of volume:

Mixed Oils.	Sp. Gr. observed.	Sp. Gr. calculated.
Lard and sperm$_{34}$	0.89736	0.89735
Sperm$_{34}$ and whale	0.89905	0.89962
Lard and whale	0.91778	0.91750

Considering the difficulty of precisely detaching measured volumes of liquids generally, and especially of such adhesive and viscid ones as oil, it may be safely presumed that the deviations between calculation and observation shown above are attributable to this source. Even were they somewhat more serious than they are, the comparison of the results would still warrant the conclusion, that in mixed oils no change of volume occurs; that when the ingredient oils are known, the proportions in which they have entered can be precisely defined; and that when they are not so known, the density of the resulting mixture will often furnish a guide to their discovery and assignment.

Accordingly, it being inferred that the H. samples, from their rank in the table of densities just given, and from certain other circumstances, were probably mixed articles, calculation was made of the proportions in which the four unadulterated oils would have to be taken, in order to yield mixtures of identical specific gravity with the H. samples, respectively, as follows:

1. *Binary combinations:*

3.05 parts whale + 1 part sperm$_{34}$: No. 1.
5.60 " lard + 1 " " : No. 2.
for H.; and
3.83 parts whale + 1 part sperm$_{40}$: No. 3.
8.64 " lard + 1 " " : No. 4.
for H$_{40}$.

2. *Ternary combinations:*

3.77 parts lard + 1 whale + 1 sperm$_{34}$: No. 5.
1.— " " + 2.5 " + 1 " : No. 6.
1.— " " + 1 " + 0.5 " : No. 7.
for H.; and
6.39 parts lard + 1 whale + 1 sperm$_{40}$: No. 8.
1.— " " + 3.39 " + 1 " : No. 9.
1.— " " + 1.— " + 0.38 " : No. 10.
for H$_{40}$.

In arranging these, sperm$_{40}$ was attributed to H.$_{40}$ from the assumed analogy of their solidifying points, and before any special experiments had been made to determine these points. With this allocation, the binary combinations above are all that can be made—the ternary ones, on the contrary, are unlimited; but those that have been selected manifestly cover the limiting conditions of the question. It will be seen, presently, how other applied tests concur with while they restrict the selection.

The mixtures, then, according to these proportions, were made with all requisite precautions and reserved for the other tests just referred to. The specific gravities of two of them, taken at random, viz: the third of the binary series and the first of the ternary, were observed at the temperature of 62° Fahr., as follows:

3. Sp. Gr. 62°. Sp. Gr. 64.5°.

$3.77\ L. + 1\ W. + 1\ S._{34} = 0.91089 \quad : \quad H. = 0.91005$

$3.83\ W. + 1\ S._{40} = 0.91182 \quad : \quad H._{40} = 0.91184$

The discord in these two results is within the correction to be applied for the differences of temperature ; and it did not seem necessary after these to verify experimentally the densities of the other mixtures.

4. *Fluidity.*—Observations were made on this upon the assumptions of cohesion being constant for the same fluid, while it probably differs in different fluids; and of such cohesion, therefore, and its reciprocal, fluidity, being indicated by the number of drops that, under identical conditions, were required to make up a constant volume. And these assumptions were strengthened by the very appreciable differences that were noticed in the method of indication first employed, viz : the usual way of dropping into a graduated glass vessel, along a straight, small, and pointed glass rod from beakers containing the oils respectively. There is no doubt, however, that for this way, the quantity of liquid in the beaker, the rate of pouring and supply, (which is almost necessarily dependent upon such quantity, varies with it, and is shown, among other things, by the irregular intervals of the drops themselves,) and the angle with the horizon at which the rod may be held, materially affect the result. Another method, therefore, more troublesome but uniform, had to be resorted to in the use of a pipette holding about a quarter of an ounce weight of the several oils, filled successively to the same level, held vertically at a constant height above the recipient. These precautions seemed to cover all that was requisite to secure uniformity in the results. The pipette, of course, had to be cleaned carefully between each set of observations, which consisted of two or three on the same oil ; and the temperatures were observed in the beakers at the moment of filling the pipette. The following were the observations :

Oil.	Temperature.	No. of drops.	Volume.
Lard	61°.25 — 61°.5	46	$\frac{1}{6}$ fluid ounce.
Whale	61° +	46	"
Sperm$_{34}$	62°.25 — 62°.50	47	"
Sperm$_{40}$	63° — 63°.50	47	"
H	63°	49	"
H$_{40}$	63° + — 63°.50	47	"
Do	133°	50 — 51	"
Do	147°	51 — 52	"

The temperatures were observed in the beginning and throughout, under the supposition that they were influential—a supposition which receives confirmation from the last two observations. Nevertheless, it was evident, both in relation to this topic and in general, that, unless with a much more elaborate and complex apparatus, the differences in the number of drops would not be sufficiently marked to offer any practically instructive result. Such a result was, therefore, aimed at otherwise.

For this an iron plate, 70.075 inches long and about 5 inches wide, was fluted along its whole length by semi-circular grooves 0.4 inch in diameter, carefully planed and polished, and as nearly as possible exactly similar. One end was then raised 0.875 inch, (corresponding to an angle of 42′ 31″ nearly,) and in each of the grooves, over a transverse zero line, ten drops of the different samples of oil were allowed to fall, respectively. The time and temperature at that epoch were noted, as also at convenient daily intervals afterwards, and at each of these successive epochs, the distance in inches run by the respective samples was read off from the graduated edges of the plate. A good many days were allowed to elapse, in the anticipation that some of the samples would become entirely stationary; then the inclination of the plate was augmented by another 0.875 in. (corresponding to an angle of 1° 25′ 2″ nearly) and the distance run read off as before at several epochs; and, finally, similar readings were made after the plate had been still farther elevated by an additional 0.875 inch, and thus inclined at an angle of 2° 7′ 35″ nearly.

All these observations are included in the following table:

Table of the distances run by the several Oils and of their corresponding intervals, etc.

DATE.	TEMPERATURES.			EPOCHS.		LARD.		WHALE.		SPERM. 34°		SPERM. 40°		H.		H. 40°	
1855.	At observation.	During maxim.	Interval minim.	Whole No. of hours elapsed.	Intervals.	Run in inches.	Velocity in inches per hour.	Run in inches.	Velocity in inches per hour.	Run in inches.	Velocity in inches per hour.	Run in inches.	Velocity in inches per hour.	Run in inches.	Velocity in inches per hour.	Run in inches.	Velocity in inches per hour.
Feb. 7 1h.30 p.m.	60°.	....	....	0.	Angular	inclinati	on=0°.42'										
8 11h.45 m.	64°.	....	47°.	22.25	22.25	8.75	0.3933	20.3	0.9124	29.2	1.3123	24.4	1.0966	24.8	1.1146	20.7	0.9303
9 11h.50 m.	64°.	....	....	46.33	24.08	19.20	0.4144	24.3	0.5245	29.9	0.6453	27.	0.5827	26.5	0.5720	24.4	0.5266
10 1h.25 a.	60°.	....	53°.	71.91⅔	25.58	23.	0.3198	25.	0.3476	30.1	0.4185	29.4	0.4088	27.	0.3754	24.8	0.3448
12 10h.15 m.	67°.5	....	57°.	116.75	44.83	25.40	0.2176	26.	0.2227	30.3	0.2595	30.4	0.2613	27.9	0.2393	25.5	0.2184
13 9h.50 m.	72°.	....	52°.	140.33	23.58	26.60	0.2183	26.5	0.2126	30.9	0.2202	31.5	0.2245	28.8	0.2052	26.5	0.1888
14 11h.20 m.	69°.5	....	58°.5	165.83	25.50	26.70	0.1610	27.1	0.1634	31.2	0.1881	32.	0.1930	29.4	0.1773	27.9	0.1683
15 11h.50 m.	69°.	....	58°.5	190.33	24.50	26.70	0.1403	27.4	0.1440	31.6	0.1660	32.4	0.1702	30.	0.1576	28.7	0.1508
16 11h.15 m.	70°.5	....	57°.5	213.75	23.41	27.15	0.1270	27.8	0.1301	31.9	0.1492	32.6	0.1525	30.4	0.1422	29.15	0.1364
19 10h.00 m.	66°.	69°.5	59°.	284.50	70.75	27.30	0.0960	28.3	0.0995	32.2	0.1132	32.75	0.1151	30.8	0.1083	29.65	0.1042
20 9h.30 m.	67°.	71°.	64°.5	308.	23.50	27.60	0.0896	28.45	0.0924	32.25	0.1047	32.8	0.1065	30.85	0.1001	30.	0.0974
21 2h.50 a.	67°.	68°.5	60°.	337.33	29.33	27.70	0.0821	28.6	0.0848	32.25	0.0956	32.8	0.0972	31.1	0.0922	30.15	0.0894
22 11h.00 m.	68°.	68°.	61°.	357.50	20.16	27.95	0.0782	28.65	0.0801	32.35	0.0905	32.95	0.0922	31.2	0.0873	30.3	0.0848
23 10h.35 m.	70°.6	70°.6	60°.5	381.08	23.58	28.	0.0735	28.7	0.0753	32.55	0.0854	33.05	0.0867	31.25	0.0820	30.55	0.0802
24 10h.25 m.	60°.75	60°.75	59°.	404.91	23.83	28.10	0.0696	28.7	0.0709	32.6	0.0805	33.10	0.0817	31.25	0.0772	30.65	0.0757
II SERIES.	Angular	inclinati	on=1°.	25'													
26 5h.20 a.	63°.5	68°.5	52°.	459.83	36.91	30.2	0.0657	28.8	0.0627	42.65	0.0928	42.65	0.0928	31.6	0.0687	32.7	0.0712
28 0h.20 a.	60°.5	60°.5	55°.5	502.83	43.	30.45	0.0605	28.9	0.0575	45.25	0.0900	....	....	31.7	0.0630	33.8	0.0689
III SERIES.	Angular	inclinati	on=2°.	7'													
Mar.1 10h.50 m.	61°.	61°.	56°.	525.33	21.50	31.8	0.0605	28.95	0.0551	61.15	0.1164	61.	0.1161	31.85	0.0606	34.7	0.0661
2 0h.30 a.	66°.5	66°.5	56°.	551.	25.67	32.05	0.0582	29.	0.0526	65.75	0.1193	65.25	0.1184	32.05	0.0582	35.3	0.0640

The captions to the foregoing table are supposed to be distinct enough to explain themselves. The range of temperatures there shown is so restricted as to be probably without material influence on the fluidity; at all events, the measure of such influence is so obscure that the temperatures themselves have merely been recorded for future reference and possible use hereafter.

The columns of distance run, unreduced for temperature, afford at once a visible contrast in both of what may be called the permanent and local fluidities of the different oils. This distinction will be recognised in comparing the last observation in the first series as between lard and sperm$_{34}$. With the former the advance during nearly twenty-four hours was 0.1 inch, and double that of the latter; but during the next interval, when the force of gravity was increased, the greater permanent fluidity of the sperm$_{34}$ is recognised by a run of more than 10 inches, while the lard advances little more than 2. But without attending to this distinction further, the number in the columns referred to, in any of the observations, furnish at once relative values for the general fluidity of the different samples. Thus, taking the last observation in each of the three series, we derive readily the following ratios of fluidity as under:

	Sperm.$_{34}$	Sperm.$_{40}$	H.$_{40}$	H.	Lard.	Whale.
1st series—ratio ······	1. —	1.01	0.94	0.96	0.86	0.88
2d series—ratio ·······	1. —	1.01	0.74	0.70	0.67	0.64
3d series—ratio ·······	1. —	0.99	0.54	0.49	0.49	0.44

The adjacent columns, however, of velocities per hour furnish a more comprehensive ratio than those which are to be derived from the different series separately; indeed, it was in view of such an application that more than one series was observed. The columns in question contain numbers in a converging series, whose apparent want of regularity chiefly arises from the want of equidifference in the epochs of observation. When the series is interpolated so as present values of velocity corresponding to equal intervals of time, this irregularity in a great measure disappears, second differences become nearly constant, and the value corresponding to any observed time may be reproduced with tolerable accuracy from any three equidistant terms, taken as the first in the series.

In point of fact, however, and in view of the labor demanded for such a double interpolation, it is near enough for the present purpose to rehabilitate any required term by taking proportional parts of the difference between the two observed terms immediately preceding it, and it is in this way that the results presently given have been calculated. But however calculated, it is manifest that if series I be continued by interpolation to the epoch of the first term of series II, we shall have a value for velocity smaller than that actual first term; and the difference between the observed term, which belongs to a greater inclination of the plate, and the calculated term, which represents what would have been observed had the inclination remained unchanged, indicates the effect produced by gravity. Of course, in this case, gravity is in fact encumbered with the friction and inspissation of the oils, respectively; but, looking no further for the moment than to series II, the first of these affections (friction) is always constant, and the second, (inspissation,) which depends upon the oxidation, and possibly other chemical changes in the oils exposed, and so may vary in the different samples, may be considered so slightly variant as to be assumed to be constant also; and thus the differences obtained may be attributed to gravity solely, and regarded as indices of the fluidity of the masses respectively.

The numerical values of the differences thus obtained are as under:

	Sperm.$_{34}$	Sperm.$_{40}$	H.$_{40}$	H.	Lard.	Whale.
Velocity observed	0.0928	0.0928	0.0712	0.0687	0.0657	0.0627
Velocity calculated.....	0.0643	0.0652	0.0608	0.0613	0.0606	0.0608
Difference	0.0285	0.0276	0.0104	0.0074	0.0051	0.0019

These differences show the influence which gravity has had in augmenting velocities, and in so far are relative measures of the characteristic (fluidity) which we are engaged in comparing.

Another set of relative measures of the same characteristic can be obtained from the observations in series III, by applying to them the same process which has just now been explained. Of this application the numerical results are as under:

	Sperm.$_{34}$	Sperm.$_{40}$	H.$_{40}$	H.	Lard.	Whale.
Velocity observed	0.1164	......	0.0661	0.0606	0.0605	0.0551
Velocity calculated.....	0.0857	......	0.0528	0.0543	0.0524	0.0496
Differences	0.0307	......	0.0133	0.0063	0.0081	0.0055

The observation of sperm$_{40}$ was, in this series, accidentally missed. The order of the other samples is the same here as in the former case, except the H., which, in falling below lard, seems to have been subject to some local irregularity of the apparatus or accidental alteration of its own constitution, of no great amount, but sufficient to exclude it from the comparisons that are going to be made of the others.

Now had these last differences resulted in the same numerical value as the former ones, it would have shown that the influence of gravity, which is theoretically equal in both cases, was actually equal in both, and that there had been no agency at work to augment or diminish that influence. Conversely, as the numerical values are different, it shows that there has been some alteration in the effect of gravity; and, the number operated upon being a converging series and to be taken with contrary signs, the higher values in the second instance show that the action has been diminished, and diminished to the extent of the difference between the two sets of values. The cause of this diminution is, of course, in the gradual inspissation of the oils; which, in the previous calculations, has been assumed as constant for all the samples, but which, it will be seen, now varies for each and finds an index in terms of velocity by subtracting the last set of differences from the former. The following is the numerical result of the subtraction as well as the ratio of the indices in question:

	Sperm.$_{34}$	Sperm.$_{40}$	H.$_{40}$	H.	Lard.	Whale.
Index of inspissation	—0.0022	May be taken as equivalent to the preceding.	—0.0029	May be taken as equivalent to the preceding.	—0.0030	—0.0036
Ratio of Fluidity...	1.	....	0.76	....	0.73	0.61

The ratio of these negative indices of inspissation, taken reciprocally positive, has here been called the ratio of fluidity; because practically, in all applications of oil requiring exposure to the air and contact with metals, (which covers nearly or quite all the uses of oils of the class now in question,) the act of thickening or inspissation is the reciprocal of fluidity. To determine absolute fluidities, *i. e.* the force of cohesion (or rather the want of cohesion) of the particles, would require a special apparatus and much attention; while, after all, such a determination would only apply in the abstract or momentarily to the particular liquid at the epoch of its being observed, and, to be made practically useful, would have to be taken in connection with the age and previous exposure of the liquid, and of its tendency to inspissate under general or special circumstances—the very topic of the present research and determination.

The absence of tendency to thicken appears of great importance in the application of oils to the purpose of illumination, quite as much or more, indeed, than in other applications to which it is generally considered as more directly interesting, such as, for instance, the lubrication of machinery; and it is for this reason that the characteristic tendencies in this respect of the different samples have been observed and discussed here so much at length.

5. *Affection by heat.—Expansion.*—The experiments on expansion were made in the following manner:

A glass bolt head, capable of holding about 10 ounces weight of oil, and divided along its stem directly to thousandth parts of its capacity, (which division had been made for another purpose very carefully,) was filled, as nearly as convenient, to its zero point, with oil of the sample under trial; and was then suitably suspended in a metal vessel holding about two and a half gallons of water. A thermometer was similarly suspended close alongside and with its bulb at the same depth with the centre of the bolt-head bulb. The water was then heated to near its boiling point, the vessel and apparatus removed to a separate chamber, and, after an average interval of three quarters of an hour, which was presumed sufficient to allow of equilibrium of temperature being established between the oil and the water enveloping it, the stand of the mercury in the thermometer and of the oil in the bolt-head stem (which became, in fact, a large oil thermometer) was begun, and, at convenient intervals throughout the day, continued to be observed and recorded. The cooling of the large mass of water was so slow that a whole day was required for each sample, and the more the cooling progressed, until the temperature approached that of the chamber itself, the more marked became the differences of temperature at different depths in the boiler. To read the thermometer, therefore, without lifting it (which was not always easy to be done) was the only precaution that had to be taken in this respect. The stand of the oil, on the contrary, altered very slowly, and the graduation for that could be very carefully and patiently examined. The thousandths marked on this scale cover each a space of rather more than $\frac{1}{16}$ of an inch, and admitted therefore of ten thousandths being estimated with tolerable precision. Perhaps it would be more just to say that five thousandths could be thus estimated; which is as near as an apparatus of this sort can be relied on with certainty. This is by no means an approximation sufficient to determine for theoretical purposes a physical constant, not being, at best, accurate beyond the fourth decimal place; while the stathmiometric method, applied already by one of us to the determination of the expansion of linseed oil, measures readily and exactly up to the seventh decimal. But the method employed here was ample for the present aim; and was besides the only one, if any were admitted at all, that could be expected to come into use in ordinary hands as a practical test among oils.

The following are the results of the four samples, which were all it was thought necessary to observe, and in the order in which the experiments were made:

Expansion of Sperm Oil.$_{34}$

Temperature.	Expansion observed.	Expansion calculated.
46°	1.0015	1.001500
51°	1.0036	1.003615
66°	1.0099	1.009960
74°	1.0133	1.013344
105°	1.0265	1.026457
125°	1.0351	1.034917
135°	1.0390	1.039147
145°	1.0436	1.043377
155°	1.0480	1.047607
165°.5	1.0522	1.052049

Expansion of Lard Oil.

Temperature.	Expansion observed.	Expansion calculated.
47°	1.0075	1.007500
70°	1.0171	1.017229
75°	1.0192	1.019344
90°	1.0256	1.025689
100°	1.0299	1.029919
110°	1.0340	1.034149
118°	1.0374	1.037533
139°	1.0465	1.046416
145°	1.0493	1.048954

Expansion of H. Oil.

Temperature.	Expansion observed.	Expansion calculated.
60°	1.0191	1.019100
80°	1.0275	1.027560
90°	1.0317	1.031790
97°	1.0343	1.034751
108°	1.0391	1.039404
130°	1.0485	1.048710
140°	1.0529	1.052940
143°	1.0542	1.054209
165°	1.0634	1.063515

Expansion of Whale Oil.

Temperature.	Expansions observed.	Expansions calculated.
61°	1.0155	1.015500
66°	1.0177	1.017615
85°	1.0258	1.025654
99°	1.0318	1.031576
105°	1.0342	1.034114
129°	1.0443	1.044268

The columns of calculated expansions affixed to these observations have been throughout founded upon one and the same factor, viz: 0.000423 per degree of Fahrenheit, derived at first from the sperm series—being the average, very nearly, of all the readings there. Had this factor been carried to a more remote decimal and used, it would have given results more strikingly accordant in the sperm and whale series, but more in excess for the lard and H. samples. It was not, however, a compromise which dictated its employment; but the conviction that it was sufficiently exact to illustrate the conclusion which all the observations warrant, viz: that although these different oils have not precisely the same factor of expansion, yet the difference between their respective factors is very minute and not readily appreciable by any practical test, so as to establish a marked distinction among them.

Cooling and Solidification.

Experiments on these characteristics were thus made: Samples of the different oils were put in spherical glass flasks of the same size and filled to exactly the same volume, viz: two fluid ounces. The flasks were then immersed in a vessel of water (so wide as to allow of their contents being clearly visible all the time) which was itself set in another vessel containing the cooling mixture. Thermometers were kept in the water-bath, as well as in one or other of the flasks, until the water congealed; when the frigorific mixture was applied directly to the flasks in the bath itself. The temperatures here recorded are those of the thermometers in the oil only; the temperature of the bath being uniformly from 2°.5 to 3° lower.

The thermometer stand was noted at the first sign of congelation; which, in some of the instances, was a milky or rather tallow-like ring, commencing to form against the sides of the flask and progressing towards the centre, and in others was a somewhat iridescent, milky pellicle, that spread itself with a certain suddenness over the entire surface of the oil in the flask. After this, the refrigeration was urged until the entire masses respectively became gelatinized or quite solid. The following table exhibits the results:

Temp.	H.	H.$_{40}$	Sperm.$_{40}$	Whale.	Lard.	Sperm.$_{34}$
40°.5	Milky ring.	----------	----------	----------	----------	----------
40°	----------	milky ring.	----------	----------	----------	----------
38°.5	----------	----------	Milky ring.	----------	----------	----------
38°	----------	----------	----------	Pellicled.	----------	----------
37°	----------	----------	----------	----------	Pellicled.	----------
35°	----------	----------	----------	----------	----------	Pellicling.
30°	----------	----------	Congealed.	----------	----------	----------
28°	----------	----------	----------	----------	----------	Congealed; white deposit.
17°	Congealed; part solid.	----------	----------	----------	----------	----------
14°	----------	Gelatinous.	----------	----------	Congealed; part solid.	----------
11°	----------	----------	----------	Gelatinizing.	----------	----------

What is here termed congelation, in the case of the two sperm oils, seemed to be not properly a solidification of the mass but a deposit of a substance resembling spermaceti, much the most copious in the winter sperm, the supernatant liquid appearing unaltered. It was of no practical interest in these, or the other cases, to prolong the experiment down to total solidification, for the deterioration of the sample for purposes of illumination was evidenced long before, and at the time of the formation of the ring or pellicle first noticed.

6. *Combustion.*—Experiments for this were made with ordinary tinned jacket lamps, as nearly as might be of the same capacity and same sized wick-holder, which were filled to the same level with what proved to be very nearly the same weight of the oils, respectively, and provided with wicks, untwisted, of the same number (30) of strands, carefully cut to the same length and set to the same height; in short, with all the arrangements and circumstances, the same for each. They were then lit, left to burn undisturbed for a certain time, extinguished simultaneously, weighed (as they had been before lighting) to determine the consumption, relit, and so on, through three series of observations, of which the following table gives all the necessary results:

Time in burning.	Sperm$_{34}$ Gr. pr. minute.	Sperm$_{40}$ Gr. pr. minute.	Lard Gr. pr. minute.	H.$_{40}$ Gr. pr. minute.	H. Gr. pr. minute.	Whale Gr. pr. minute.
125 min.	1.7998	1.3874	1.3686	1.3225	1.3215	1.3054
320 "	1.7814	1.3725	1.3616	1.3371	1.3126	1.3232
440 "	1.7013	1.3036	1.3154	1.3075	1.2619	1.2809
Avr. consump'n.	1.7608	1.3545	1.3485	1.3224	1.2987	1.3032
Ratio.	1.—	0.76925	0.76585	0.75102	0.73756	0.74012

The rate of combustion being a function of the fluidity of the oil, is in a measure an index of fitness for purposes of illumination. In the foregoing table, it is seen that all the samples, except the whale and H.$_{40}$, show a rate diminishing directly with the time in burning. These two, only, show in the second interval an increased rate. This apparent anomaly arises, no doubt, from the state of the wick during combustion. Thus, the observations applicable to the whole period of burning were, that—

Lard oil in combustion leaves a jagged, irregular wick, with frequent points here and there remaining at a red heat.

Whale oil gives rise to remarkable fungose, sometimes bonnet-shaped processes of unconsumed carbon, which, as they are growing, render dim, but when they fall off at intervals, spontaneously or otherwise, refresh the light.

Sperm oil, both winter and summer, burns with a clean, even wick, pointed here and there in white.

H. oil exhibits the same phenomenon as whale, only not quite so abundantly.

H.$_{40}$ burns with a wick like the other H., with the phenomenon of, here and there, red points, like those observed with lard.

These bonnets of unconsumed carbon, which are peculiar in their excess to the samples of whale and H., and visibly affect the regularity of illumination, affect, no doubt, with similar irregulatity the rate of combustion, and thus in some degree account for the anomalies of the table.

The state to which the wick is reduced by combustion in it of the different samples, is further indicated by the relative facility in relighting after extinction, upon which the following observations were made :

	Order of facility :—	First relighting.	Second relighting.
Sperm $_{34}$	No. 1	lights	instantly.
Sperm $_{40}$	No. 2	do.	very easily.
Lard	No. 3	do.	easily.
H.	No. 4	do.	hardly.
H. $_{40}$	No. 5	do.	very hard.
Whale	No. 6	do.	with greatest difficulty.

A numerical index of the degree of coaling of the wick, evidenced for the different samples respectively in the last two classes of observation, cannot be derived from them at all; but some approximate values may be obtained by comparison of the rates of combustion before given. This can only be done in four of the cases; the remaining two (whale and H.$_{40}$) have been already noted as anomalous, and are too irregular for calculation.

It is true that, in general, the diminishing rate of combustion is attributable in part to the diminishing supply of oil in consequence of the lowering of the surface of the reservoir and the increased labor of the capillary fibres, as well as to the carbonized and more impervious state of the wick. But the effect of the first of these causes cannot be very material in the present instance; and we may therefore, for approximation, consider the successive diminutions as arising from the condition of the wick, and compare and proportion them accordingly.

If, then, the amount of coaling and deterioration of the wick were the same with all the samples, the proportion of the differences of consumption in the two intervals of the three observations would be constant for all; and, of course, if the amount of coaling varies in the several samples, the said proportion will vary directly with it, and thus serve to indicate the varying amount.

Now, in fact, for the four calculable cases, the differences in question and their proportion are as under :

	Sperm_{34}.	Sperm_{40}.	H.	Lard.
Differences of consumption in grains per minute....	0.0184 : 0.0811	0.0149 : 0.0689	0.0889 : 0.0507	0.0070 : 0.0462
Proportion	1 : 4.4	1 : 4.6	1 : 5.7	1 : 6.6
Relative coaling of wicks—	1	1.05	1.30	1.50

Such are the approximations of their characteristic, which will be referred to again presently.

7. *Illuminative power.*—The experiments on this were made with a Ritchie photometer, in which the rays from a standard light and from that under comparison, at opposite ends of a long box, are caught upon two equal and similar mirrors placed within, back to back, at angles of 45°, and thus reflected vertically upwards through a suitable opening over the junction of the mirrors, where they are tempered by passing through a screen of uniform tissue paper. Of course, the squares of the distances of the two lights from the junction of the mirrors, when the screens are equally illuminated, represent the relative intensities of the light. The standard employed was a candle of Judd's patent sperm sixes, which burn with great uniformity and are very proper for such an application. The oils were burned in the lamps with the wicks and precautions already described. The lamps were lit at three-quarters past six o'clock, on the morning of the 12th of February ; four hours afterwards the experiments were commenced ; an hour and a half was required for their completion; so that the mean time of burning was nearly five hours, which was fixed upon as a fair average period for developing the properties of the combustibles, both in respect to illumination and to their modes of combustion. As far as the lard oil, however, is concerned, this period was undoubtedly too prolonged; for its wick had become so jagged and crusted, and its light so dim, that, except for form's sake, it was wholly unfit to be introduced into the comparison. It should be said that none of the wicks had been touched or disturbed during the whole period.

After one set of experiments had been gone through with, the photometer was turned end for end, and another set performed—not so much for confirmation's sake, or as a test of the instrument which had been before ascertained to be tolerably reliable, but for the purpose of a special comparison, which will be spoken of hereafter. In comparing the intensities of the light, the homologous terms of the

ets were multiplied together for an average ratio, instead of
ing the terms of each set separately and averaging between
. The arithmetical division of these products, respectively,
the value of the respective lamp flames in terms of the stand-
andle light, as shown in the fifth column of the following table:

OILS.	PHOTOMETRIC DISTANCES.		PHOTOMETRIC DISTANCES.		Lamp in candle.	Ratio of intensity.
	Lamp.	Candle.	Lamp.	Candle.		
	Inches.	*Inches.*	*Inches.*	*Inches.*		
4	9. 875	10. 75	8. 2	10. —	0. 75326	1. —
0	9. —	10. 75	7. 25	10. —	0. 60698	0. 8058
	5. 8125	10. —	6. —	12. 4375	0. 28040	0. 3723
	5. 9375	10. 75	5. 75	12. 50	0. 25407	0. 3373
	6. —	17. 5625	6. —	20. 375	0. 10060	0. 1336
il	6. —	38. 75	6. —	40. 25	0. 02308	0. 0306

e last column of the preceding table gives merely the ratio of
tensities of the light of the several lamp flames, respectively,
ut reference to the quantity of oil consumed uniformly in main-
g the flames and producing their light. It is obvious that upon
uantity depends the economical applicability of the respective
les, or what may be called their *specific illuminative power*, which
ectly, as the intensities themselves, and, inversely, as the quanti-
onsumed in a given time.

ducing, then, these intensities in the ratio of average consump-
respectively, as given in a previous table, we obtain a final
t, as under :

Oils.	Photometric ratio.	Average ratio of consumption (p.	Economical ratio.
4	1. —	1. —	1. —
0	0. 8058	0. 76925	1. 04750
	0. 3723	0. 75102	0. 4957
	0. 3373	0. 73756	0. 4573
	0. 1336	0. 74012	0. 1805
il	0. 0306	0. 76585	0. 0400

is to be observed, upon the results of this table and the preceding, that while, other things being equal, the illuminative power is directly in the ratio of the consumption, yet circumstances, in being changed and unequal, may so modify the character of the combustion as, with an equal or even greater consumption of oil, to yield less light. Thus, the consumption of lard oil is nearly as great as that of summer sperm, and of whale oil greater than that of the H. sample ; while the ratio of the light given in the respective cases is vastly disparate.

The probable explanation of such instances is to be found partly in the peculiar condition of the wick, which, itself carbonizing in such manner as not to offer a mechanical obstacle to the supply of the oil near its surface, yet presents it to or shields it from oxidation, by modifications of temperature or otherwise, in such a way as to allow the carbon of the oil to be burned without becoming highly luminous. It would be interesting, in a practical point of view, to have this topic investigated more fully.

But, without attempting to pursue it much further here, a comparison of the photometric distances just now given will serve to illustrate the subject. The contrast of different conditions of wick in the different samples, which was made in the last section, was founded upon the mechanical aspect only of interference with the capillary supply of the combustible. The difference of the distances just now mentioned, shows that the peculiar coaling of the wick, in the case of lard oil especially, affects the light in a different mode. This will be observable in the following table :

Oils.	RATIO OF PHOTOMETRIC DISTANCES.				Difference of ratios.
	First series.		Second series.		
	LAMP.	CANDLE.	LAMP.	CANDLE.	
Sperm_{34}	1 :	1.0886	1 :	1.2195	0.1309
Sperm_{40}	1 :	1.1944	1 :	1.3793	0.1849
H._{40}	1 :	1.7204	1 :	2.0729	0.3525
H.	1 :	1.8105	1 :	2.1739	0.3634
Whale.	1 :	2.9271	1 :	3.3958	0.4787
Lard oil	1 :	6.4583	1 :	6.7083	0.2500

Of course, if during any given period of burning, the condition of the wick is unaltered, the ratio of the 'photometric distances at the beginning and end of such period, will be unchanged. Also, with the same wick, if the same combustible be used, although the ratios at the beginning and at the end will vary, their differences will be constant and in the same sense. And when with different combustibles and the same wick the differences are irregular, it seems that it would be possible, by proper observations, to assign to each element its proper share in the result and to determine how much of the irregularity is due to deterioration in the supplying power of the wick, and how much to the peculiar affection of the individual combustible in being burnt.

Of course, also, in this determination, the chemical constitution of the combustible, which, in what has been so far said, has been considered as unascertained, plays a part more or less important, according to the proportion of carbon and residual hydrogen which the sample may contain. But this topic will come up more suitably in the succeeding portion of the memoir, which concerns—

CHEMICAL CONSTITUTION OR INTERNAL PROPERTIES.

1. *Homalism.*—Among these was first examined the homalic state of the different samples, *i. e.*, the presence or absence of any uncombined acid or alkali, as evidenced by indifference to test paper stained with litmus and immersed in them.

None of the samples gave any reaction upon such a test, either immediately or within an hour and half. Observed again after twenty-four hours, the two samples of sperm manifested a very faint acid action, which was less marked in sperm$_{34}$ than in the other. The remaining samples, after this long exposure, were still absolutely neutral.

But when they were boiled in a long test-tube, at the mouth of which slips of test-paper were suspended in the vapor, there occurred a very slight acid reaction in all the samples, showing that the temperature of boiling oil (about 600° F.) is sufficient to overcome the normal affinities in the substance and liberate some undetermined acid which ordinarily remains combined.

2. *Qualitative associations.*—A general examination was made for the detection of acids and basis that might exist in any of the samples, and especially in the presumed factitious ones of H.

There were found very slight traces of chlorine in all; and similarly slight traces of potassa in all, except the lard oil. Such traces do not allow of any comparative scale or estimate of quantities as probably existing in the respective samples, and fail, therefore, to lead immediately to any instructive result in the way of furnishing means for determining either the fact or the extent of adulteration.

3. *Quantitative composition.*—Nor was such instruction expected to be found directly in the determination, by organic analysis, of the proportions of ultimate elements constituting the different oils, which, however, with a special view that will be referred to more particularly hereafter, was made for the three natural oils and resulted as under:

Ultimate elements.	Lard oil.	Whale oil.	Sperm$_{34}$ oil.
Carbon	0.76658	0.77511	0.76490
Hydrogen	0.10586	0.11430	0.12150
Oxygen	0.12756	0.11059	0.11360
	1.—	1.—	1.—
Residual hydrogen	0.089915	0.100476	0.107300

These analyses were not extended to the summer sperm or to the H. samples, partly on account of the tediousness and costliness of the operation, but chiefly because the differences between the extremes of any one of the elements in the ascertained cases were so small as not to yield directly any decisive result.

Before leaving this point of investigation, however, it was of course desirable to ascertain how far the numerical results of the analyses were reliable, and to determine their probable limits of error for the content of carbon, at least, which element presents the chief difficulty of accurate elimination. As for the hydrogen, there is so much less likelihood, both in theory and in fact, of its apparent quantity being affected by the accidental errors of the process, that such apparent quantity may safely be taken for the true one.

This being assumed, the ascertained specific gravities furnish a scale whereby the probable errors in the quantity of carbon may be assigned. In applying this, the apparent quantities of the several elements, expressed in proportionate weights, were reduced to volumes, and the factor of contraction in bulk from the gaseous elements to the liquid oil, ascertained for each sample.

These volumes were all numerically different, as might be expected, standing, in fact, in the following ratio, viz:

Sperm.$_{34}$	Whale.	Lard.
1.—	0.973	0.938

But when these came to be combined with the respective specific gravities of the several oils, in order to obtain the factors for contraction, the ratio of said factors was different, and stood actually as under:

Sperm._{34}	Whale.	Lard.
1.—	1.024	0.978

How far these differences connect themselves with the mode of combination of the several elements, and serve to indicate important differences of nature (so to speak) in different oils of nearly identical ingredients, would be an interesting research for general purposes; but it was not called for here, and the only use that has been made of the indications was to assume that the factor of contraction is constant for all samples; that its apparent variations proceeded from errors of experiment, and that the average of all the variations would represent its true mean value. This mean value was applied to correct the original number of volumes, and the ratio of said original and the corrected number was taken as the ratio between the observed and the probable limiting quantity of carbon in the respective oils. The calculation of the quantities resulted as under:

	Sperm._{34}	Whale.	Lard.
Carbon found······	0.76490	0.77511	0.76658
Carbon calculated··	0.76118	0.75854	0.78367

The differences between experiment and calculation, amounting to nearly 1⅔ per cent. on the oil for whale and lard, and ⅜ per cent. for sperm, show that the probable content in carbon may vary according to circumstances for the three kinds of oil between the limits as under:

	CONTENT IN CARBON,		
	Maximum.	—	Minimum.
Lard oil ·········	0.78367	—	0.74949
Whale ··········	0.79168	—	0.75854
Sperm_{34} ·········	0.76862	—	0.76118

Such are all the particulars that appear necessary to be stated here in reference to the item of chemical composition. What follows is the result of divers comparative tests, intended for fixing the relation to the natural oils of the (supposed) factitious H. samples.

4. *Chromatic tests.*—These are judged of, as the epithet implies,

by the color struck in given masses of the oils, respectively, (contained, for instance, in a test-tube,) by given and equal volumes of different acid and alkaline reagents. The several reagents here employed were nitric and sulphuric acids and caustic soda in solution ; which last was boiled in the test-tube with the oils. Aqua regia (nitro-muriatic acid) with soda was also applied ; but it gave no instructive indications.

A good many preliminary experiments were tried, both with the several samples and also with mixtures of the different natural oils in various proportions, in order to determine the most advantageous strength of the acid reagents. It is not necessary to give the details of these ; but it may be said that sulphuric acid of a density higher than 1.475 (as for example, of 1.53 and 1.635) is too strong, carbonizing the mass so deeply as to destroy all characteristic grades of hue ; while, on the other hand, nitric acid of density less than 1.22 failed to attack decidedly either the sperm or lard oils.

The following table gives the result of experiments, with the acids, etc., adopted upon all the samples. The mixtures were in definite volumes ; and the unitary volume of acid employed (one fluid drachm) was throughout constant. The volume of oil was likewise constant, and equal to five fluid drachms. The volume of soda only was ten fluid drachms. After the mixture was uniformly shaken, it was left to rest for five minutes, and the color, both of the oil above and of the acid substratum below, observed and noted. Although the hues of the upper and lower portion of the mass were different, yet their relations in the several samples were found to be always the same, except in one instance (with sulphuric acid) where the anomaly was so slight as to be insignificant. The sequence in the table is of hues darkening as they succeed.

5 VOL. OIL MIXED, ETC., WITH

1 vol. Sulph. acid (1.47.)	1 vol. Nitric acid (1.22.)	1 vol. Nitric acid (1.33.)	1 vol. nitric acid (1,33.) 10 vol. soda (1.34.)
Lard oil.	Lard.	Lard.	Lard.
Sperm.$_{34}$	Sperm.$_{34}$	Sperm.$_{34}$	Whale.
Whale.	H.$_{40}$	Sperm.$_{40}$	H.$_{40}$
H.	Sperm.$_{40}$	H.	H.
Sperm.$_{40}$	H.	H.$_{40}$	Sperm.$_{34}$
H.$_{40}$	Whale.	Whale.	Sperm.$_{40}$

The inference from these experiments, like that derived from all the preceding tests is, that H. samples are intermediate between sperm and whale oils, and are, therefore, probably a mixture of the two.

To test this inference more precisely, elaborate series of experiments were next made in a similar manner upon these H. samples, and upon the ten mixtures whose proportions are given on page 6, whose external properties, in general, strongly resemble more or less those of the H. samples, and whose specific gravities are respectively the same. These mixtures were, for greater distinctness and impartiality, numbered in the order in which they occur on the page already quoted, and were referred to and known only by their numbers.

It is not necessary for the present purpose to give the details of the experiments, which had to be made with great minuteness, and show the gradually darkening shades of the several samples throughout, as just now exhibited in the former group. It will be enough to present the main result of identity or near equality of color between the H. samples and some one or other of the mixtures with the various reagents, respectively, as follows, the volumes, etc., being as already mentioned :

1 volume sulphuric acid, sp. gr. 1.475 with 5 volumes oil :

After 3 days, H. = H._{40} — = No. 1 = No. 3 —.

1 volume nitric acid, sp. gr. 1.33 with 5 volumes oil :

After 5 minutes, H. = No. 1.
“ 3 days, H. = No. 1 = No. 3, nearly.
“ 5 minutes, H._{40} = No. 10.
“ 3 days, H._{40} = No. 9 = No. 6, nearly.

1 vol. nitric acid, sp. gr. 1.33, and 10 vols. soda, with 5 vols. oil :

After 40 hours, H. = No. 9, nearly.
“ “ “ H._{40} = No. 3, nearly.

But the shades were so slightly marked in this series, that the observations were recorded at the time as not conclusive.

1 volume soda, sp. gr. 1.34, with 5 volumes oil :

After 15 minutes, H. = H._{40} + = No. 3.
“ 3 days, H. = H._{40} + = No. 1 = No. 3.

Neglecting those equations which are merely approximate, the results may be more perspicuously grouped, as under :

	Sulph. Acid.	Nitr. Ac. immed.	Nitr. Ac. 3 days.	Soda.	
H.	No. 1	No. 1	No. 1	No. 3	
$H._{40}$	No. 3	No. 10	No. 9	No. 1	No. 3.

With the first and the last of these reagents the two H. samples were themselves but faintly distinguishable; and, as might have been expected, their differences are denoted by opposite signs. If we resort, then, to the specific gravities, and couple their indication with this last table, it may be concluded with tolerable certainty that the H. sample is the same as No. 1, and that the $H._{40}$ can only be No. 3, No. 9, or No. 10. If mixtures be made according to the formulæ of these numbers, they will be hardly distinguishable, by any of the tests hitherto known, from the H. samples, respectively.

5. *Thermal Tests.*—An analogous result is secured by another kind of test, to be spoken of now. These tests should be, in theory, absolute; they are, however, in fact only comparative. They repose upon the observation made some time since for certain vegetable oils, but here, for the first time, applied to animal oils, viz: that when definite quantities of oil and of sulphuric acid, of a certain density, are mixed, the mass exhibits a rise of temperature which is nearly uniform, both in its maximum and in its range, after a given short period, (two and a half or three minutes,) and is characteristic of the oils themselves, respectively.

After this had been observed, very numerous experiments were made to determine the proper density and quantity of the sulphuric acid to be used, in order to yield the greatest differences in the characteristic temperatures. In regard to the first of these points, acid of 66° Baumé at 60° Fahrenheit, corresponding to a tabular density of 1.767, was taken as the most advantageous; and in regard to the latter, the temperatures were found to rise regularly with the three natural oils, lard, whale, and sperm, as the proportion of acid by volume was increased from one-twelfth to one-half of the oils, respectively. When this last proportion was reversed, so as to make the volume of acid twice that of the oil, the resulting temperatures were found to decrease.

When mixed in the proportion of 2 fluid ounces of oil to 1 of acid, the normal rise above the initial mean temperature of the mass was ascertained to be, in—

Lard oil,	78°	Fahr.	at a maximum, and	74°	after	2½ minutes.
$Sperm_{34}$	69°.5	"	"	66⅔°	"	"
Whale	128°.5	"	"	120°	"	"
$Sperm_{40}$	*66°.25*	"	"	63°.5	"	"

These constants are the mean of several observations, the individual variations of which, for lard and sperm_{34}, were but a fraction of a degree of Fahr., and as the whale series reached in one instance three degrees. In this last, however, the greatest deviation from the adopted mean is but a degree and half. The observations after a given interval appeared less discordant, and would be preferable for use in this respect, were it not that in practice they complicate the experiment. Nevertheless, they are all to be looked on yet as provisional only.

Next, trials were made in a similar manner upon mixtures in various proportions of whale and sperm oils, to see if their characteristic temperatures were maintained in alligation, with the following results :

OBSERVED.			CALCULATED.		
Sperm_{34}.	Whale.	Max'm temp.	Max'm temp.	Whale.	Sperm_{34}.
0.975	0.025	68°.125	70°.98	-	-
0.950	0.050	69°.—	72°.45	-	-
0.900	0.100	72°.25	75°.40	0.047	0.953
0.750	0.250	81°.25	84°.25	0.199	0.801
0.700	0.300	84°.25	87°.20	0.250	0.750
0.500	0.500	92°.9	99°.—	0.400	0.600

This table, the mean of many observations whose individual differences were within the margin before mentioned, fully sustains the principle of characteristic temperature preserved in alligation ; although from some want of precaution in manipulation or defect in observation, or error in the characteristic constants employed, the actual and calculated results are not the same. Their differences are, however, regular ; and it would not be difficult to construct an empirical formula from these observations, or to apply corrections by which these differences would disappear.

But without any such corrections, if the H. sample, whose average maximum rise of temperature is $107\frac{2}{9}$°, with a variation of 1°.5 between the highest and lowest observation, be calculated strictly, it results in the following proportions, viz : Whale, 0.639 ; sperm, 0.361. The proportions of the artificial sample No. 1, which is identical in specific gravity and the other tests already mentioned, are, whale, 0.763 ; sperm, 0.247. The smaller proportion of whale oil accused by the thermal test, would lower the specific gravity from 0.91005 to 0.90546. In the methods procured for ascertaining specific gravity, there cannot be so great an uncertainty as this; and, therefore, the whole or greater part of the error must lie in the preparation of the mixtures and the mode of observing the temperatures. So far, therefore, this test is merely confirmatory of the comparative results of the preceding without imparting much more absolute precision.

In the case of the H.$_{40}$ sample, however, it does somewhat more, by showing that there must be a third ingredient in the mixture. The maximum rise of temperature with this sample, averaged, is but $89\frac{1}{10}$°, with a variation of 2° between the highest and lowest reading. This temperature would correspond to a mixture of whale 0.367 ; sperm$_{40}$ 0.633; having a specific gravity of 0.89503 instead of 0.91104 belonging to the H.$_{40}$ sample. There must have been, therefore, some ingredient which adds to the specific gravity while it reduces the rise of temperature. Now, lard oil, which has a thermal function of only 78° Fahr., while its specific gravity is greater than that of H.$_{40}$, answers to these conditions ; and it becomes easy to determine among the mixtures, just now mentioned as analogous or identical with H.$_{40}$ in all the other tests, which it is that coincides or comes the nearest in this one. Of these, there are but two triple mixtures, No. 9 and No. 10, which need be considered. The former of these should have by calculation a thermotropic maximum, or, what may be called for convenience, an epitherm of 107°.5 ; the latter an epitherm of 97°.4 instead of 89°.1 actually observed in H.$_{40}$ The variation of this latter falls within the margin which there was just now occasion to observe in the trial mixtures of the table last given ; and there is, therefore, reasonable probability for inferring, upon all the several tests, that the mixture No. 10 and the H.$_{40}$ sample are identical.

If any one should ask what would be the effect upon the other tests in throwing all the stress upon this one, calculation shows that a mixture containing one half the volume of whale oil less than is in No. 10, and differing 0.00214 in specific gravity, would have an absolutely identical epitherm; while observation of the numerous arbitrary mixtures tried in the course of this research, authorizes it to be said that the chromatic tests heretofore spoken of would not be seriously affected by the diminution in question.

There is, however, a chromatic test first suggested in the course of these experiments which would be very sensitive, not to the diminution in the whale oil, indeed, but to the consequent increased proportion of lard oil residing in the mixture. This increase is in the ratio of 0.421 to 0.533, or more than 11 per cent., while the test itself is sufficiently delicate to discriminate readily down to 5 per cent. of lard oil in a mixture whose ingredients are otherwise unchanged. It still remains to be determined, however, by a series of experiments, whether this test is specific for lard oil under all circumstances and with any mixtures. Under the assumption which generally holds good, that the adulterants of sperm oil are only whale and lard oils, it undoubtedly applies to the latter with convenience and precision. This test, then, distinguishes the substitute mixture now in question, (consisting of 1 part lard oil + 0.5 whale + 0.375 sperm$_{40}$) both from No. 10 and from H_{40}.

The theory of the thermal action which has been just now spoken of, is obscure and does not explain the cause either of the differences in the initial rise of temperature or epitherm with the different oils, or of the differences in the successive epitherms of the same oil observed in the earlier epochs of the experiments, when the relative proportion of acid used was small. These differences are evidenced in the following paradigm of the mean of several observations :

Mixture by Volume.		Mean Epitherm.		
Oil.	Acid.	Lard Oil.	Sperm$_{34}$	Whale Oil.
1.	0.083	22°.7	28°.4	40°.4
1.	0.167	53°.7	53°	103°.9

Passing this, however, and taking only the later epochs of the experiments, under the assumption that the products of combination in the mixture are sulphurous acid, water and varying hydro-carbons whose conditions are yet unascertained, successive epitherms may be calculated, with a tolerable approximation, on the hypothesis that they are respectively as the water produced, directly, and as the aggregate of the specific heats of the several products inversely. The following shows the worth of such calculation :

WHALE OIL.		SPERM.$_{34}$		LARD OIL.	
Calculated.	Extremes observed.	Calculated.	Extremes observed.	Calculated.	Extremes observed.
113°.4	114°.9 — 116°.6	64°.4	67° — 67°.5	70°	71°.3 — 71°.5
121°.2	122°.9 — 124°.5	67°.9	69°.3 69°.5	----	---- ----
134°.1	127° — 130°	71°.—	70°.3 70°.5	76°	75°.8 76°

In respect to another point observed in these thermal tests, viz: the existence of a maximum rise of temperature corresponding to certain proportions of acid, the calculations indicate the reasonableness of this in showing that at or about the point taken for such maximum, the aggregate specific heat of the mixture, which practically increases in a higher ratio than the caloric liberated by the union of the oxygen and hydrogen, has become such as to compensate for the actual, increase of caloric so liberated. Of course, the sensible temperature, under these conditions, becomes for a while nearly stationary, and then falls agreeably to observation.

It can hardly be doubted that further study of these analogies, and others which have not been mentioned, will furnish a clue to a definite and satisfactory theory, capable of supplying a perfect test for the distinction and determination of these natural oils in whatever combinations they may exist.

J. H. ALEXANDER.
CAMPBELL MORFIT.

University of Maryland,
Baltimore, *June*, 1855.

SECOND REPORT

ON

OILS:

ADDRESSED TO

THORNTON A. JENKINS, ESQ., U. S. N.,

U. S. LIGHT-HOUSE BOARD, ETC.

Thornton A. Jenkins, Esq., *U. S. N.*,
U. S. Light-House Board, &c., &c., &c.

Baltimore, *October* 15, 1855.

Sir: I have the honor to present the accompanying report of Professor Morfit, of the University of Maryland, and myself upon certain oils transmitted to me for examination early in the current year, by your direction; and upon some which came to hand for a similar purpose and in the same way, only recently. A sample of so-called corn-oil, which came among these last, having been, with your approval, withdrawn as being unnecessary to be examined. The report now sent in covers, with the former, all the samples of oil that have been at any time submitted to me.

The principles and methods of examination applied here have been the same with those adopted in the former report, as far as they go, only, the motive and aim being here not precisely the same as on the former occasion, there has been a corresponding modification in the extent and character of the research. There the object was chiefly, by minute comparison of small differences, to determine identities; here, it is rather to ascertain the salient points of distinction and the comparative practical values of samples professedly different; there, the problem was to find out the probable composition of certain specimens; now, it is to examine into the relative fitness of certain other specimens for the purposes to which they are sought to be applied. I hope that you will recognize these distinctions as having been properly drawn, and the results, in the two cases, as being, respectively, appropriately obtained.

For all these present samples, then, the density, fluidity, both characteristic and at special low temperatures, the rate of combustion, the tendency to deteriorate the wick and the direct illuminative power of each, are all the points strictly necessary to be observed. Some of these even reciprocally involve the others. Thus, the tendency to deteriorate the wick is calculable from the rate of combustion; and with the divers unadulterated animal oils that I have tried (and probably with all, as well as with unadulterated vegetable oils, compared as a separate class and upon their own scale,) the illuminating power marches at the same rate, but inversely, with deterioration of the wick, and any one of a class being taken as a standard, may be arithmetically deduced with tolerable approximation from it.

Yet care has to be taken that the experiments for the most of these be performed under like circumstances. A slight change of temperature, say 5° F., perceptibly affects the rate of combustion in such lamps as I used; and a small difference in the height of wick, which might be expected to produce a difference in illumination, is as well marked in the quantity consumed in a given time. The experiments, to be properly comparable, seem to require a height of wick appropriate to each sample and as great as possible just not to smoke. Also, I think I have found that this height, not to affect the results, must be obtained by gradual movement in the same direction upwards; a wick once in contact with flame, even for a brief period, and then drawn down again into the tube, no longer performs its normal functions as a capillary pump. It happens sometimes, indeed very often, that a wick set to a certain height and smokeless for a while, begins afterwards to smoke. This will always be found to arise from some projecting fibre in the interior of the flame where the air has less access than elsewhere. It is hardly necessary to say that the proper remedy, at least the one resorted to in the present instances, is to cut off such fibre or fibres and not to lower the wick. The tendency of a wick to smoke is ascertainable with reasonable precision in the beginning by holding over the flame, at a distance above it, a small sheet of clean metal by which the draught is dampened and its carbon-depositing tendencies multiplied. If this distance is considerable, say 12 or 15 inches, and yet carbon is deposited on the sheet, the lamp will be very apt to smoke spontaneously after the sheet has been withdrawn.

In connexion with the rate of combustion, and specially the photometric results which are more or less dependent upon it, I must not fail to observe that the economical values that are given in the last table under the head of these results have no relation, as might possibly be inferred from the epithet used, to cost. They refer altogether to the quantities of combustible used, which are, for this term, equated throughout, so as to compare the effect of equal quantities in equal times. This equation is made upon the hypothesis that with the same oil the illumination is in proportion to the quantity consumed in a given time, the other circumstances being equal. Thus, if any oil, burning at the rate of one grain per minute, gives a certain amount of illumination, the same oil, with a flame produced by the burning of 1.5 grains per minute, will give half as much more light. In point of fact, all the other samples but two consumed less per minute than the sperm, and all their actually observed indices of illumination have to be and have been, therefore, raised to what they would have been had their quantities consumed been as great as that of the Robinson sperm. The actually observed index of illumination, on the other hand, of the samples, lunar and non-gelatinous lard oil, whose consumption was greater than that of the Robinson sperm, has to be and has been lessened just in the proportion of the respective consumptions.

It is in this way, then, that the physical merits, expressed in terms of light furnished by equal quantities in a given time of the respective samples, have been prepared to admit application of their commercial merits, expressed in terms of price of a given quantity, in order to a determination, whenever desirable, of the question of relative cheapness. For example, if the economical photometric value of any oil B is 50 per cent. of that of another oil A, whenever the price of B is one-half that of A, the two fluids are, for the purposes of mere naked illumination, equally cheap; if B is less than half-price, it is the cheaper, and so on. There are, of course, other considerations when, as is the case with lenticular lights, the illumination is not naked, but enclosed, which enter in and control the question of cheapness. But this question is not among those referred to me, and I have been unconsciously led to saying as much as I have said upon it.

In respect to another topic connected with combustion—the deterioration of wick, various with different oils—the method that has been used for its numerical determination is new and, in proportion to the accuracy with which the several dimensions are taken, reliable. In the present instances I must not be regarded as offering more than tolerably approximate results. In the aspect in which these present samples are regarded here, their chemical constitution is not of so much importance. No quantitative analysis, therefore, has been made of them, because I considered that the probable practicable advantage in having its results did not warrant the certain expense. The differences in the ultimate elements of the different samples could be but small, and, in all likelihood, would be hardly more instructive as to the mode of compounding the factitious oils than the qualitative examinations in that view which have been made. In a purely scientific and theoretical point of view it would be very interesting to have a full analysis of the seal and colza oils; but the use to be made of such a knowledge would be, for some time to come, altogether general and abstract, and I could not allow myself, therefore, without a special authorization, to burden your appropriation with what could not yield a direct corresponding practical benefit.

It is possible that you may think it of interest hereafter to pursue the colza research, whose unexpected inferiority in all the samples submitted is visible throughout the experiments, and is yet to me, in the face of different results elsewhere, inexplicable. Should this be the case, I recommend that these samples and others, if there is any doubt of the genuineness of these, (in view of which it was principally that the report has stated the source and date of acquisition of the several samples,) should be tried in advance by other methods of burning, as mentioned in the report.

Finally, I refer to the discussion of thermal tests, with which the report closes, as containing an unexpected corroboration of the convenience and reliability of the particular test, by epitherms, which I before had the honor to recommend, being introduced into the system of inspection of your supplies. And I respectfully suggest now that if, upon consideration, you should be favorably disposed to such introduction, it would be serviceable to collect in advance as great a variety as can conveniently be procured from respectable dealers of reputed unadulterate sperm, (both winter and fall,) with a view to the determination of their respective densities, epitherms, and illuminative powers, in order to know the margin which actually exists in these respects between the best and worst specimens of the same natural, unadulterated article, and in order, besides, to ascertain, if possible, the relation which undoubtedly connects the phenomenon of the epitherm with the illuminative power of any particular sample.

At present, you are aware, the intended use of the epitherm is only to assure us of the identity of the whole of any lot with its sample. It is possible that, by the means which I have suggested, it may be susceptible of more extended and advantageous application.

In general, I am very slow in committing myself to any step which leads to trouble or expense. In the present case I do not apprehend there will be much of either, while the probable prospective results are of high importance. I have, therefore, no hesitation in mentioning them as I have done.

Submitting this, then, and the other accompanying recommendations to your consideration, I have the honor to remain, sir, your obedient servant,

J. H. ALEXANDER.

REPORT

The oils to be spoken of in the following report were from samples submitted at different times and examined at various periods. The particulars of name, date of receipt, and number affixed to each, necessary for distinctness and identification, are conveniently grouped in one table, as under:

SAMPLES.		OBTAINED—	
No.	Name.	When—	From whom—
1	Colza$_1$	Jan. 23, 1855	A. L. Case.
2	Colza$_2$	Mar. 26, 1855	Do.
3	Colza$_3$	July 13, 1855	Do.
4	Lunar, (Aug. Yockney)	Aug. 22, 1855	L. H. B.
5	Mason's patent	Jan. 23, 1855	A. L. Case.
$5^{\text{bis.}}$	do	July 18, 1855	L. H. B.
6	Non-gelatinous lard, (Theo. Adams)	Aug. 22, 1855	Do.
7	Rosin	Jan. 23, 1855	A. L. Case.
8	Seal	Jan. 23, 1855	Do.
9	Solar	Jan. 23, 1855	Do.
10	Sperm, (Curtis, Mitchell & Co.)	Jan. 23, 1855	Do.
11	Sperm, (E. M. Robinson.)		

Of these, No. 11 is of the same sample designated as sperm$_{34}$ in the first report; and some of the results (those, to wit, which are presumed to be independent of local circumstances, temperatures, etc.) obtained there have been again employed here. No. 5^{bis} was supposed to be a specimen identical with the previous one already examined for the Light-house Board, and still more minutely, chemically, before for another party; and was, therefore, upon our suggestion, officially withdrawn. All the others were regularly and uniformly tested; more or less continuously, as the successive results seemed to require, and as will be specifically noted under the several heads hereafter.

The order of the examination is the same here as in the former report; only some of the items there given, and which were important for the special discrimination aimed at then, are omitted here as not essential to the more practical comparison intended now. And the motives and principles of the tests applied, the mode of applying them and the apparatus for their application, were then so fully discussed and described as to render a repetition of such details unnecessary, and to allow this report to be confined chiefly to the numerical results obtained, and to their comparison under their several appropriate items. In this way, this becomes justly a sequel to the former report; while, of course, any instances in which conclusions hitherto considered as established have come in these later observations to be modified will be noted in their proper place.

PHYSICAL PROPERTIES OR EXTERNAL CHARACTERS.

Density.—The following table exhibits the specific gravity, or density compared with water, of the different samples, observed at different times and at varying temperatures, but reduced to a uniform temperature of 62° Fahr. The elements of this reduction are: for water, the data derived from the experiments of Mr. Hassler by one of us, and given in Silliman's Journal, (Sec. Ser. xvi, 170;) and for oil, the factor 0.000423 per degree of Fahrenheit, established in our former report:

	Samples.	Observed at—	Densities at 62° F.
	Water		1.
No. 1.	$Colza_1$	$67\frac{1}{3}$° F.	0.91408
2.	$Colza_2$	73	0.91049
3.	$Colza_3$	73	0.91413
4.	Lunar	73	0.91644
5.	Mason's P	$68\frac{2}{3}$	0.91906
6.	N. G. lard	73	0.91729
7.	Rosin	69	0.97182
8.	Seal	$67\frac{2}{3}$	0.92489
9.	Solar	68	0.92066
10.	Sperm, Mitch	$68\frac{1}{3}$	0.87878
11.	*Sperm,* Robins	$64\frac{1}{2}$	0.88060

Fluidity.—The experiments on this were made with the same grooved apparatus described in the first report, but in two sets, at different epochs, when the temperatures also happened to vary materially. Three series, of as many different inclinations, were also taken as before; but, in consequence of accidental inattention, these series were not made of the same number of terms, respectively, and, when finished, were found to be not exactly comparable, but at one epoch of time and under one inclination. A comparison of the results, however, as obtained, seems to warrant our taking the one where the period of time elapsed and the inclination were identical, as sufficiently indicative of all; while the point in question did not appear of practical importance enough, in its present relation, to demand a repetition of all the sets and series of experiments. The co-incident result only, therefore, is presented, as under:

	Ratio of fluidity.	Observed at.
$Colza_1$	0.618	57° mean.
$Colza_2$	0.921	75° "
$Colza_3$	0.826	" "
Lunar	0.671	" "
Mason	1.978	57° "
N. G. Lard	0.828	73° "
Rosin	0.700	57° "
Seal	0.945	" "
Solar	1.856	" "
Sperm, M.	1.	" "
Sperm, R.	1.	75° "

All these relate to a uniform period of 71.5 hours and a constant angular inclination with the horizon of 42 minutes of a degree. The Robinson sperm, which is regarded as a standard, was intended to be compared simultaneously in both sets; but, owing to a casual intromission of some foreign substance into the groove containing said sample, it failed of its function during the first set; and is, therefore, rather arbitrarily, assumed to be equivalent there to the other, Mitchell sperm, which it so closely resembles in some other properties, and to have been identical in both sets. That it is not exactly so would appear from comparison of these later experiments with those of the first report, in which all the other circumstances, except the temperature, were the same, nearly. Thus:

1855, Feb. 10. Robinson sperm after 71.9 hours, mean temp. 56°.5 stood 30.1 in.
1855, Sep. 1. " " 71.5 " " " 75° " 43.2 in.

It appears from this that the temperature is a much more important element than has been hitherto supposed in the various methods that have been contrived for experimenting on this characteristic of fluidity; and that to determine its theoretical functions would require, among other things, a precedent determination of the delicate differences in the expansion by heat of the several oils, which expansion has undoubtedly erroneously been taken, nevertheless with a sufficient approximation for practical purposes, as identical for all.

It is to be observed, however, that whatever may be the issue of this point it affects with error the ratio of fluidities only to the extent of the difference of behavior under changes of temperature, degree for degree, between the Robinson and Mitchell sperms—a difference that may be assumed to be ordinarily hardly perceptible.

This discussion of temperature leads naturally to the next characteristic of—

Affection by heat, one aspect of which, viz., expansion, was not considered necessary to observe, after what had been done and said in the first report; the other aspect, that of—

Cooling and solidification, has been experimented upon for these samples in the same manner and under the same precautions as for the former, and with the following results:

Experiments upon Cooling and Solidification.

Temperature.	Colza$_1$.	Colza$_2$.	Colza$_3$.	Lunar.	Mason, P.	N. G. Lard.	Rosin.	Seal.	Solar.	Sperm, M.	Sperm, R.
°											
45							Pellicle.				
44				Thick pellicle.		Thick white pel.					
43				Solidifying in nuclei.		Solidifying					
40										Pellicle.	
35	Pellicle.	Lt. pellicle.	Lt. pellicle					Pellicle.			Pellicling.
33										Solid.	
32					Pellicle.			Very viscid.			
31.5		Thick pellicle.	Thick pellicle.								
31							Viscid				
28.5								Solid			
26.5									Pellicle.		
26	Viscid.										
25					Solidifying pt. solid						
23		Thick milky.	Thick milky.								
22		Mass thickened less than C$_3$.	Wh. deposit on therm.								
21	Very viscid.								Viscid.		
19	B. to solidify.										
17							Very thick.				
12									B. to solidify		
9									Not quite solid		

Combustion.—In the experiments for this, the best of three samples of colza only was taken, and the rosin burned with so thick and unavoidable a smoke as to render it totally unfit for purposes of illumination. The following table exhibits the important particulars with the rest:

Samples.	1st period=90′. Consumption in gr. per min.	2d period=230′. Consumption in gr. per min.	3d period=320′. Consumption in gr. per min.	Average consumption in gr. per min.	Ratio.
Colza$_2$	1.5301	1.3686	1.2446	1.3811	0.80521
Lunar	1.9045	1.7013	1.4672	1.6910	0.98589
Mason	1.6363	1.5147	1.4238	1.5249	0.88965
N. G. Lard	1.9477	1.9031	1.7903	1.8804	1.09632
Seal	1.1584	0.9986	1.0283	1.0618	0.62042
Solar	1.2101	1.1829	1.1345	1.1758	0.68552
Sperm, Mitchell	1.6459	1.5358	1.4223	1.5347	0.89476
Sperm, Robinson	1.7493	1.7219	1.6745	1.7152	1.

Connected with these results and influencing them was mentioned, in the first report, a—

Carbonization and Coaling of the Wick, as characteristic of different oils. This topic has been since examined, and will be further treated of here. For this, one of the lamps employed in the experiments above, all of which were intended to be of the same dimensions and pattern in all respects, and do not appear to vary very materially, was measured carefully in its upper and lower diameters—it being a conic frustum—and in the height from the top of the wick-holder 1° to the normal surface of the oil when the lamps were full, and 2° to the bottom when the lamps were empty. The difference between these last is the effective height of the frustum of oil, when full. These dimensions were as follows: Upper diameter = 1.5 inch; lower diameter = 2.2 inch; height of frustum = 1.9 inch; further height from top of frustum to top of wick-holder = 1.1 inch. From these dimensions, the capacity of the lamp ($= Q^{\text{c. inc.}}$) in cubic inches, at different levels, may be expressed in terms of the height (h) counted from the top downwards to said levels; and consequently, if those capacities, *i. e.*, the volume of oil burned at different periods, be known, the corresponding value of h may be calculated by the following formula, the numerical co-efficients of which are near enough for the present purpose, viz:

$$Q^{\text{cub. in.}} = 1.7659h + 0.4341h^2 + 0.0355h^3.$$

This formula was considered as applicable to all the lamps, indiscriminately.

Next, from the known specific gravities of the oils, respectively, and from the known weights consumed of each during the three periods respectively given in the last table, it is easy to deduce the several volumes 9_1, 9_2, 9_3, consumed during those periods; and with the formula to calculate the several corresponding depressions of surface h_1, h_2, h_3, at the end of each of those periods. The half of these depressions is the average depression during the whole period respectively, and such half added to the constant height ($1.1^{\text{in.}}$) between the original normal surface of the oil and the top of the wick-holder where it is delivered for combustion, gives a series of values, H_1, H_2, H_3, which correspond to and represent the average capillary action required in the wick during each of the periods aforesaid. Of course, H_2 being greater than H_1, and so on, as the surface of the oil sinks, there is a corresponding increase of capillary work to be done by the fibres of the wick in (as it were) pumping up the oil for combustion, and a proportionate diminution in the quantity of oil consumed.

If the actual consumption in the several periods were found to be inversely in the ratios of $H_1 : H_2 : H_3$, they would show that the diminished consumption was due solely to the increased difficulty and spontaneous deficiency of the supply; but if, as was throughout these experiments and always is the case, the diminutions are in the higher ratio that $\frac{1}{H_1} : \frac{1}{H_2}$, etc., it is plain that there are some other causes affecting either the delivery or the combustion, or both.

Here, from the data at disposal and probably from the nature of the case generally, it is impossible to distinguish among these causes; and they are therefore treated in the aggregate and comprehended under their two main aspects, by the terms *carbonization* and *coaling;* the former being considered to refer to a diminution or destruction of the wick itself, whereby its surface and consequently its capacity of discharge are lessened; and the latter, to an incrustation upon said wick, different in different oils, whereby the delivery and the combustion are both impeded.

Now, to calculate this aggregate, it is sufficient to consider that there are actual consumptions, expressed, say, in grains per minute, and denoted by g_1, : g_2 : g_3, etc., in a converging series, and values 1 :

$1 + n : 1 + n'$ etc., in a diverging series, resulting from the fractions $\frac{H_1}{H_1} : \frac{H_2}{H_1} : \frac{H_3}{H_1}$, etc.. and corresponding to the increased capillary action demanded. The value of the first term of the converging series being taken as normal, we have the values affecting the succeeding terms otherwise than by capillarity, and denoted by c and c′, obtained by the equations following:

on (A) $c = \overline{1 - n} \,.\, g_1 - g_2 =$ carbonization, etc., dur'g 2d period.

(B) $c + c' = \overline{1 - n'} \,.\, g_1 - g_3 =$ do. 2d and 3d period.

$c' = \text{Eq. B} - \text{Eq. A} =$ do. 3d period.

The numerical values to be substituted for for g_1, g_2, g_3; are given in the last table; those of capillary height (H_1, H_2, etc.) and their ratios, $\overline{1 + n}$, $\overline{1 + n'}$, and finally of c and c′, the amount of carbonization, etc., expressed in grains per minute, have been calculated, and are presented in the following table:

Samples.	Weight of cubic inch	Capillary depth.			Ratio of capillarity.			Carbonization, &c.	
		H_1.	H_2.	H_3.	$H_1 + H_1$.	$(1 + n.)$	$(1 + n'.)$	2d per. (c.)	3d per. (c′.)
	Grains.	*Inch.*	*Inch.*	*Inch.*				*Grains.*	*Grains.*
Colza$_2$	229.737	1.2570	1.4315	1.5045	1. —	1.1388	1.1969	0.1391	0.0803
Lunar	231.252	1.2910	1.4970	1.5690	1. —	1.1596	1.2099	0.1707	0 1748
Mason, P	232.009	1.2660	1.4585	1.5495	1. —	1.1363	1.2239	0.1050	0.0599
N. G. Lard	231.504	1.2950	1.5385	1.6450	1. —	1.1880	1.2700	0.0662	0.0787
Seal	233.524	1.2195	1.3470	1.4295	1. —	1.1046	1.1722	0.1432	—0.0632
Solar	232.261	1.2245	1.3890	1.4705	1. —	1.1343	1.2009	0.0236	0.0369
Sperm, Mitch	222.163	1.2750	1.4765	1.5660	1. —	1.1580	1.2282	0.0926	0.0800
Sperm, Rob	222.163	1.2825	1.5150	1.6325	1. —	1.1813	1.2749	0.0224	0.0318

The carbonization, ect., during the first period of observation cannot be deduced from these nor readily from any observations. It has to be assumed as null; and, therefore, in any experiments of the kind undertaken for final precision the first period should be as brief as possible, and at least very much shorter than the term which, in view of other more interesting results, was allowed here. But the ratio of the results for the two last periods, which results besides conform to and illustrate all the mechanical phenomena exhibited during the experiments, enables us to analogize in some degree the characteristic occurrences and relations for the first.

Thus, in the instances of colza$_2$ and Mason's patent, it is seen that *the greatest* deterioration of wick occurred during the second period.

The carbonization is from 50 to 100 per cent. more in this than in the third. And it may therefore be inferred that the characteristic tendency of both these oils is to coal quickly, and that a considerable carbonization had already taken place during the first period. With the lunar and Mitchell sperm such deterioration appears nearly constant during the whole time, while in the other cases, except the solar oil, the wick grows gradually less efficient from period to period, and, as may be inferred, from the moment of lighting onwards. The seal oil only presents an anomaly—which is also accused in the Table of Combustion, and, indeed, arises out of the observation there of a larger quantity consumed during the third than during the second period—in giving a negative carbonization, thus indicating that the wick got better instead of worse, and became more efficient, to the extent of $\frac{6}{00}$ of a grain per minute, at last than at an earlier period. If necessary, this anomaly is not difficult of reconcilement. The seal oil has the tendency, already spoken of with regard to others in the first report, to form hoods or bonnet-like excrescences on the wick, which visibly affect the illumination, and no doubt the consumption of combustible, which at intervals fall off, refresh the light and, of course, the vigor of consumption.

The Robinson sperm, undoubtedly the best of all the samples, maintains its precedence also in this aspect as producing or allowing the least deterioration of wick. But it is to be observed that the indications in this regard are not to be taken exclusively, nor at all except in connexion with other circumstances, such as capillary depth, viscidity, fluidity at various temperatures, &c., which influence the result. Indeed, the whole speculation under this topic may be considered as more curious than useful, nearly the whole of it being practically involved in and solved by the observations, to be quoted under the next head of—

Illumination.—These observations were made in the same manner and with the same standard candle as those in the first report. The product of the respective distances, the photometer being turned end for end, is used for the ratio of illuminations only, the samples being observed (purposely with a view to another point) always in the same order, instead of being reversed, there is a slight apparent advantage for those which came the earliest. This advantage is in a less deterioration of wick by about fifteen minutes' burning between the earliest—the Robinson sperm—and the last, the solar oil. Except that the two sperms came first, the order of the others is the same alphabetical one which has been used in all the preceding tables. A column in the following table, however, for each series, showing the number of minutes from the commencement of the period to the epochs of the two observations, will show all this more particularly.

In view of the extent required for exhibiting all the details, it is enough to give for each series the proportionate illuminating power of the lamp flames, respectively, that of the candle being called unity; or, as the caption has it, the numerical per centage of "lamp, in candles," and the ratio of those illuminating powers. This is done in the following table :

Samples.	First period of 90 min.				Second period of 230 min.				Third period of 320 min.			
	Epochs.		Lamp, in candles.	Ratio of illumination.	Epochs.		Lamp, in candles.	Ratio of illumination.	Epochs.		Lamp, in candles.	Ratio of illumination.
	m.	m.			m.	m.			m.	m.		
Colza$_2$	63	82	0.2561	0.34144	210	222	0.0624	0.09065	....		....	....
Lunar.	65	83	0.6365	0.84861	211	223	0.2428	0.35276	....		....	....
Mason, P.	66.5	84.5	0.5925	0.78993	213	224	0.3732	0.54228	304	313	0.2022	0.41045
N. G. Lard	67.5	86	0.5740	0.77578	214	225.5	0.3101	0.45069	305	314	0.1136	0.23072
Seal.	70	87.5	0.3533	0.47107	215	226.5	0.2005	0.29139	306	315.5	0.2842	0.57700
Solar	71	89	0.3000	0.40000	216	228	0 2686	0.39025	307.5	317	0.2154	0.43731
Sperm, M	61	80	0.7216	0.96216	206.5	220	0.4796	0.69688	293.5	312	0.3593	0.72943
Sperm, R	58	79	0.7500	1. —	205	219	0.6882	1. —	292.5	310	0.4925	1. —

At the beginning of the third period, the colza$_2$ and lunar oils, which had already in the second exhibited a very low illuminating power, burned so badly and shed so feeble a light as to be altogether unfit for comparison. They are therefore omitted in the last three columns. The cause of this inferiority with colza, which in France is reputed to give such good results, cannot be stated other than hypothetically, in the sample being an adulterated or deteriorated one. The fact itself, as shown in these three series, is indubitable, and is further confirmed by two other series with the same sample and four others with colza$_1$, and colza$_3$. The sample colza$_2$ was selected for this last comparison because of its previously ascertained though slight superiority. How far a different mode of burning, as, for instance, with a draught of air through a glass chimney, or some means of aiding its weak and early-impaired capillary flow, as by hydrostatic pressure or mechanical delivery, there was no opportunity for ascertaining. No doubt, such appliances would remedy in some degree its defects and afford a better result.

The seal oil exhibits here the same anomalous result in respect to illumination as before in regard to combustion and carbonization. It gave a better light, just as it consumed more with less deterioration of wick, in the last than the preceding period. This will be seen more readily in the following table, which gives the ratio of illumination for the respective samples through the three periods; the amount in the first being taken as unity:

Samples.	Ratio of illumination.		
	First period.	Second period.	Third period.
Colza	1. —	0.24360	....
Lunar	1. —	0.38141	....
Mason's P	1. —	0.62988	0.34121
N. G. Lard	1. —	0.54035	0.19798
Seal	1. --	0.58078	0.80436
Solar	1. —	0.89517	0.71793
Sperm, Mitch	1. —	0.66466	0.49784
Sperm, Rob	1. —	0.91753	0.65668

A similar accord between the different results, thus independently obtained, holds also for the other samples in general; for the natural animal oils—the sperms and seal—this accord is nearly numerical, as will be seen by the following statement:

	Illumination results.	
	Observed.	Calculated from carbonization.
Robinson's sperm	1 0.92 0.66	....
Mitchell's sperm	1 0.66 0.50	1 0.67 0.47
Seal	1 0.58 0.80	1 0.49 0.77

The other oils, which are either vegetable, as colza, or artificial, as all the rest, cannot be expected to present this accord and strict proportion with pure sperm; for the light-giving power, although to some extent indicable and controlled by the physical properties, such as fluidity, tendency to carbonize, etc., is yet more dependent upon the chemical constitution of the oil and the actual condition of the hydrocarbons it contains. These dependencies, unfortunately, allow themselves to be more readily conceived of and admitted than understood, and are more easily spoken about than explained.

It will be enough, then, under this topic to give a final comparison of the economical values of the different samples; which value is directly in the ratio of observed illuminating power and inversely as the actual consumption for the different samples, respectively. To avoid a confusing multiplication of statements, these values will only be given for the first and last periods; the former representing the relations when the wick is yet clean or recently trimmed, and the latter the conditions after a continuous burning of five hours. After such an interval, the state of the colza and lunar lamp light is such as to exclude them from comparison:

	Economical values.	
	First period.	Third period.
Colza,	0 39	....
Lunar	0.78	.. ..
Mason's P	0.84	0.48
N. G. Lard	0.70	0.22
Seal	0.71	0.94
Solar	0.58	0.65
Sperm, M	1.02	0.86
Sperm, R	1.—	1.—

Such are the particulars necessary to be stated under this head.

CHEMICAL CONSTITUTION OR INTERNAL PROPERTIES.

Homalism, or the presence of any uncombined acid or alkali in the samples, was severally tested as before. Immediately there was an acid reaction by colza; in all the others the test papers remained unchanged. After the lapse of two hours, a slight acid reaction was manifested in the two other samples of colza and in the Robinson sperm; the others were unchanged. And a similar result was shown after rather more than seven hours' exposure. They were not compared in this respect, at a high temperature; the observations for the former report being considered sufficient to establish the fact of acidity being always developed in boiling.

Qualitative associations.—These exist and are determinable, except in one samplo, only by traces; which are denoted, according as large or small, by the letters *l* or *s* immediately after the name in the following table.

Colza$_1$	gave	Chlorine		Soda
Colza$_2$	"		Potassa	Soda
Colza$_3$	"			Soda
Lunar	"		Potassa, *ss*	
Mason	"		Potassa in larger proportion than traces.	
N. G. Lard	"	Chlorine	Potassa	
Seal	"		Potassa	
Solar	"	Chlorine, *s*	Potassa, *s*	
Sperm, M.	"		Potassa, *s*	Soda, *l*
Sperm, R.	"	Chlorine, *ss*	Potassa, *ss*	

The Robinson sperm is inserted here from the observations made for the former report. The rosin oil, which had shown itself entirely unfit for burning, was not examined in these relations.

The associations given above are probably more accidental than normal. But the behavior of the samples under the proper tests for these, and otherwise, enables us to assign to the factitious oils in the list the propable mode of their having been compounded. Thus, the lunar and non-gelatinous lard oils are substantially lard oil itself; the latter, more particularly, treated with turpentine or some volatile resinous oil, increasing fluidity. The solar oil, on a ticket which accompanied it, is called solar or lard oil; but this must be an error. It is a fish oil, and probably clarified whale oil. Mason's patent is also most likely a whale oil, treated with turpentine, benzole, and pearlash.

Whatever, however, may have been the methods, and whatever the design in the treatment of these samples, it is clear that they have failed to be successful in approaching the physical properties or practical advantages of unadulterated sperm.

Thermal tests.—These were applied as before, and gave the respective epitherms, or constitutional rise of temperature, as follows:

Sample.		Epitherm.	
Colza$_1$	=	97°.0	Fahr.
Colza$_2$	=	91°.3	"
Colza$_3$	=	96°.5	"
Lunar	=	73°.25	"
Mason's P	=	89°.5	"
N. G. Lard	=	75°.75	"
Rosin	=	53°.0	"
Seal	=	131°.0	"
Solar	=	117°.5	"
Sperm, Mitch	=	82°.0	"
Sperm, Rob.	=	70°.0	"

The epitherm of the Robinson sperm formerly ascertained, the mean temperature before pouring being 74°.5 Fahr., was 69°.5. The present result is 70° F. above a mean temperature of 78° F. The Mitchell sperm gave an epitherm so unexpectedly different—viz, 82° above a mean temperature before pouring of 86° F.—that it was thought necessary to repeat the observations. They gave an epitherm of 80° above a mean temperature of 63° F. The theory of the epitherm is yet obscure; but it is probable that hereafter, in the use of this test, it will be found proper to apply a correction proportioned to the temperatures of the oil and acid before pouring.

But however this may be, these repeated observations serve to show the reliability of this peculiar test, and its constancy for the same sample under different circumstances. So far as the Mitchell sample is concerned, either the test is to be taken as showing very emphatically an inferiority, which was hardly to have been expected in an article which professes to be unadulterate and of the best quality, readily obtainable in the New York market, and which besides was so well marked in some other properties as to warrant its being regarded in the early part of this investigation as an equivalent to the standard Robinson sperm, or, if the two sperms are to be regarded as equivalents, the wide margin between 70° and 82° of epitherm cannot but be alarming to the confidence which hitherto, and upon apparently reasonable grounds, we have been disposed to place in the test. In this dilemma it will be instructive to compare the actual numerical results furnished by the other tests for the two samples of sperm, respectively, as is done in the following statement:

Characteristics.	Sperm, Rob.	Sperm, Mitch.
Density	0.88060	0.87878
Fluidity, (assumed)	1.—	1.—
Solidification, incipient, at	35° Fahr.	40 Fahr.
Rate of combustion, per minute, 1st period	1.75 grains.	1.65 grains.
Do. do. 2d do.	1.72 "	1.54 "
Do. do. 3d do.	1.67 "	1.42 "
Carbonization, &c. do. 2d do.	0.02 "	0.09 "
Do. do. 3d do.	0.03 "	0.08 "
Ratio of illumination, do. 1st do.	1.—	0.96 "
Do. do. 2d do.	1.—	0.70 "
Do. do. 3d do.	1.—	0.73 "
Deterioration of wick, do. 2d do.	0.92	0.66 "
Do. do. 3d do.	0.66	0.50 "
Economical value, do. 1st do.	1.—	1.02 "
Do. do. 3d do.	1.—	0.86 "
Homalism	1.—	1.— "
Associated elements, residual	Chlorine.	Soda.
Epitherm	70° Fahr.	82° Fahr.

It will be recollected that, in its theory, the epitherm is to be counted as progressing inversely with all the other characteristics except solidification and carbonization; and it will be seen in the statement above that it does actually so progress with the exception in the single item of density, in the estimation of which, the numerical difference is so slight as to be capable of being masked by possible errors of observation and of correction for temperature.

Upon this remarkable accord, then, of so many distinct and independent results, actually obtained and consistent with theory, it seems proper to infer that the epitherm, so far from being regarded as discordant, erroneous, or unreliable, is rather to be considered as possessing the advantage of showing at once, more emphatically and upon, as it were, a larger scale, differences which, in the observation of the other characteristics, are more minute or have to be more circuitously deduced. And we are led to the conclusion that the Mitchell sample, if unadulterated, (as, from its low density, is most likely the case,) is nevertheless an inferior quality of sperm, and not to be preferred when an article identical with what has been here taken as the standard can be procured.

J. H. ALEXANDER.
CAMPBELL MORFIT.

THIRD REPORT

ON

OILS:

ADDRESSED TO

THORNTON A. JENKINS, ESQ., U. S. N.

U. S. LIGHT-HOUSE BOARD.

THORNTON A. JENKINS, Esq., *U. S. N.*,
U. S. Light-House Board, *&c.*, *&c.*, *&c.*

BALTIMORE, *January* 28, 1856.

SIR: I have the honor, herewith, of transmitting the report of Professor Campbell Morfit and myself upon the samples of oil for light-house use which you recently sent to me for examination.

The examination has been made of these samples in the same manner as of the former ones which have been before reported on, and in the statements of results those formerly obtained with the first sample furnished by Mr. Robinson, and contained in the first report, have been also introduced to serve as standards of comparison.

From these results it appears that all the samples are of fair winter sperm, except No. 2, No. 5, No. 6, and No. 8.

No. 5, a sample from Plumb Island light-house, delivered in the spring of 1854, is a fair summer sperm oil.

No. 2, a sample from Sankaty Head light-house, delivered in the past autumn, is a winter sperm which has been adulterated, accidentally, or by design. Nearly all the tests, physical and chemical, those which depend upon the point of congelation, the rate of consumption in burning, and illuminative power, as well as the chromatic and thermal analogies, point to whale oil as the probable adulterant. And the epitherm, taken with the margin that I have before spoken of as proper, in the present state of knowledge, to be attributed to this test, indicates the proportion of whale oil in the mixture as between seven and twelve per cent. Burned normally, its direct photometric value is not quite eighty per cent. of that of standard sperm, and a little over ninety-two per cent. of the worst of those which have been classed as tolerable in the present series.

No. 6, a sample of so-called signal oil, from Mr. I. S. P., of Buffalo, N. Y., is avowedly an artificial oil. The results of tests, given by Mr. P. himself in a letter, of which you have caused me to be furnished with a copy, do not differ materially, as far as they go, from those obtained in the present examination. Thus, Mr. P. gives the density of his oil 0.92 at 60° F.; we found the density of the sample at 50.5 F., to be 0.91940, a difference which is favorable to the character of the oil. Mr. P. says further that it does not solidify till the temperature sinks below 20° F.; we found the sample gelatinous at 20.5° F., and at 17° F., solidified.

I do not know if I rightly understand the third attribute ascribed by the inventor to this oil, viz: that of not undergoing decomposition until 612° F. This temperature, which is about the boiling point of fixed natural oils in general, and is, therefore, the epoch of their decomposition, was frequently attained in the application of divers of the tests used, at least the sample was frequently made to boil; but as this point of boiling is without interest in oils intended for illumination, its precise thermometric stand was not observed. If the inventor means that there is no material vaporization until the whole mass boils, then this also is borne out by our observations, which result in showing that there is no appreciable mixture of a volatile substance in the sample.

As to the composition of this factitious oil, the color and odor of the sample indicate at once its principal ingredient to be lard oil. And in this indication, besides the tests of density and temperature of congelation already spoken of, all the others accord, with the exception of the rate of consumption in burning. In this last characteristic, it stands the second of the present series, and is within six per cent. of standard sperm. But its exhibition of such a high rate, though tolerably constant, is very brief; its light is extinguished spontaneously, much sooner than that of any of the samples; and in the quantity of unconsumed residue after extinction it stands preeminent. In none of the others does this residue exceed twenty-three per cent. of the original quantity in the lamp; this sample leaves not less than thirty-nine per cent. Standard sperm leaves hardly four per cent., and that, principally, in the wick.

From the odor, another partial ingredient may be assigned, viz., some fish oil, and the chromatic test gives this as sperm.

In direct photometric value, it is next to the worst of the lot. It burns for a longer time with a better light and higher photometric value than pure lard oil; but the behavior of the wick during combustion is quite similar to the case of lard. Like this last, it is favorable to the formation of bonnets and hoods upon the wick, which burn off and are replaced by new ones, elsewhere; thus yielding, even during its short period of tolerable brilliance, a variable light. This characteristic carbonization of wick is quite peculiar to these two oils.

The epitherm of this sample is the highest of the series, and *considerably higher than* that of lard; thus indicating at once an oil of low illuminating capacity. And this indication is precisely borne out, numerically, in the direct experiments on the two separate characteristics.

Although you desired an ultimate analysis to be made of this sample, I have taken the responsibility of pretermitting it as yet, under the conviction that the instruction to be derived from it would not be proportionate to the trouble and expense of the operation. I believe that when you have looked at the tabular results which follow, you will approve of my caution, and concur with me in thinking that an analysis is not called for. Should you, however, determine otherwise, upon your advices to that effect, it will be promptly executed and discussed, and the results forwarded to you.

No. 8, a sample furnished by H. S. K., of New York, is also evidently an artificial oil, consisting, as appears from a combination of tests, of lard oil, as a main ingredient, and also of a large proportion of some volatile oil.

The specific gravity of this compound is nearly two per cent. less than that of lard oil; its epitherm and illuminating capacity, shortly after lighting, almost identical. Nearly identical, also, is its low rate of consumption during combustion; but this, instead of being followed, as in the case of lard oil, and of sample No. 6, by early extinction, is protracted through nearly twenty hours, and until all the oil in the lamps is exhausted. In this respect, it is almost, if not quite, as good as the best sperm, and in so far better, that it resists for about five hours longer the hardening and inspissating effect of the atmosphere.

This peculiarity was very interesting, as showing that the addition of the volatile oil, whatever it might be, had imparted a permanent fluidity to the mixture far above what lard oil in itself possesses, and had also relieved the wick, in great measure, from the incrustations and obstacles which lard oil alone creates. And it was, of course, a probable inference that, if the supply of combustible to the flame were augmented by mechanical means, an efficient and economical light might be obtained from this oil, which is so free from the objections and difficulties to which pure lard oil is justly liable. Accordingly, I began to experiment with it in a carcel lamp; but the observations came to a very abrupt termination; and I think myself fortunate in having been able to close them with sufficient quickness to avoid a serious explosion.

The existence and proportion of this volatile oil had been already determined by slow evaporation. The sample was now submitted to a more urgent heat, and, at a temperature below that of boiling water, it emitted a highly combustible vapor, which, under suitable intermixture with the atmosphere, would become readily explosive. Of course, this property unfits it, whatever might have been its merits in other regards, for light-house purposes, and renders unnecessary any further discussion of its behavior to the various tests.

In a former report—the second—upon a series of oils of which several samples of colza formed a part, the results came out very unfavorable, even upon the best of these samples. Thinking that these results, which were different from those ordinarily said to be attained with colza, might depend in some degree upon the manner of burning, and so would be modified by increasing the quantity of supply to the wick and the rate of combustion, I have since repeated the experiments with the sample denoted as colza$_2$ in the report referred to, and compared them with similar ones, executed at the same time and under similar circumstances, upon standard Robinson sperm.

The combustion was had and the light measured in a small, single-wick carcel lamp, capable of burning from twelve to fifteen grains of oil per minute. The ordinary chimney was used; and being without a damper, the only regulation of the volume and color of the flame was in the adjustment of the wick and the elevation or depression of the stage carrying the glass chimney. These were so managed and varied as to produce, in the two cases, the best flames respectively, without regard to the other circumstances of either, except that the base of the flame was kept always at the same height above the table, so as to fall upon the same part of the photometer.

In consequence, I suppose, of the shortness of the chimney compared with the virtual height obtained in light-house lanterns by the additional sheet-iron tube that joins the glass chimney; and, at all events, in accordance with the strict injunctions of the lampist—Gould, of this city—who originally furnished and subsequently repaired the lamp for me, the wick was elevated more than in the ordinary mechanical lamp of light-houses. This elevation was, from the crest of the burner to the comb of the wick, for the sperm 0.45 inch, and for the colza rather less, say 0.42 inch. The height of the shoulder of the chimney above the comb of the wick was, for the sperm 0.125 inch, and for the colza 0.200 inch. These adjustments produced, as far as I could judge, the largest and whitest flames, respectively.

After burning untouched for more than five hours, the lights were extinguished and the wicks examined. The sperm wick was found to be carbonized and crusted to a depth of 0.1875 inch; the lower part was unaltered. The colza wick was carbonized black to a depth of 0.25 inch below the comb, and the remainder, down to the crest of the burner, was brown; but there was no crust nor any hard points in the carbonized part, as with the sperm. The browning of the lower part appeared a natural consequence of a peculiar shrinking of the comb of the wick, which was manifested from the very first, and drew the entire wick over into a dome-like shape. The following are the numerical results:

Time—epochs.		Lamp, in candles.		Photometric ratio.	
Sperm.	Colza.	Sperm.	Colza.	Sperm.	Colza.
Min.	*Min.*				
40	40	16.81	10.16	= 1	0.604
190	....	15.70	....	= 1	0.612
....	215	....	9.61		
....	310	....	9.23	= 1	0.693
320	....	13.32	....		

Although it was intended, and would have been more satisfactory, that the time-epochs, *i. e.*, the periods after lighting up, should have been the same for the two samples, yet circumstances prevented this identity. The difference of the periods, however, is so small as not to produce any material disparity.

I did not say before, although it follows from having spoken of but one lamp, that the experiments were not simultaneous, but successive, and, in fact, on two succeeding days. But the circumstances of atmospheric pressure and temperature were the same, and I consider the results more comparable than if they had been made at the same time with two different lamps.

The comparisons were made respectively with one of Judd's patent candles, six to the pound, as in former instances. This is the only element of uncertainty in the results, and I am not disposed to consider this as material in its effect. The consumption of spermaceti is very regular, viz: 1.98 grains per minute; and I am satisfied that, by having the wick of the same height and the pool of melted matter around its base of the same size at the several epochs of photometric measurement, a reasonable degree of identity in the flame, both as to volume and illuminating power, is attained. Nevertheless, some more constant and uniform light is still to be desired. I mean, of course, something always on hand and easily accessible. An absolute standard, permanent in all times and places, can be had (though it has never been proposed yet) by burning olefiant gas, issuing from an orifice of constant diameter, under a constant pressure. But this is not of ready resort, for the gas would have to be prepared for the purpose.

For comparison of different lights and reduction to the same scale, provided the nature of the inquiry allows the experiments to be compressed in a not very long period—say, not exceeding a day—and where the place of experiment is a large city, the ordinary illuminating gas furnishes the best standard. Measured both as to volume and pressure in a special metre close by the burner, and kept uniformly regulated, it yields a light sufficiently constant until there is a fresh supply to the general gasometer. This light would be constant all the while if the supply was produced from the same materials in the same proportions and at the same temperature. But these conditions cannot be relied on longer than from day to day, and thus produce the limitation just now mentioned.

It will be seen in the tabular results that the value of the lamp in candles, or its illuminating power, goes on diminishing as the period of burning is longer. This is different from the result affirmed by some observers, who find that the lamp-flame (of a carcel lamp) increases in brilliancy through a period of five hours' continuous burning. Such an improvement, which is asserted in several books upon lighting by various authors, (but all copying, there is reason to believe, from one another, or, at least, from one and the same source,) is so inconsistent with the probabilities arising from the undoubted physical changes in the wick, that, even if its existence had been determined by a more unobjectionable comparison than was had in what I believe to be the original experiment, I should have considered the apparent result as affected by some certain though undiscovered error, and should have discarded it accordingly. I only refer to this point here lest you might think, from a recollection of some of these published authorities, that the present statement and result needed justification.

The last column of the statement gives the direct photometric value of the colza flame in terms of the sperm flame taken as unity. The progressive increase in this value is, I am satisfied, real, and not apparent only, both because the difference about the last epoch was very noticeable in advance of the photometric measurement, and because a difference in the same direction would be naturally dependent on the respective states of wick which have already been spoken of.

The mean of these several direct values may be taken as exhibiting the fair ratio of colza and sperm flames during a period of five hours. This mean results as 0.636 for colza, sperm being unity. But this is only the experimental illuminative ratio. To obtain the specific ratio of illumination, regard must be had to the quantities of material burned in the same time, respectively. This quantity is—for sperm, 14.4375 grains per minute, or 866.249 grains per hour; and for colza, 12.8496 grains per minute, or 770.977 grains per hour. The ratio of these quantities gives the proportionate consumption of colza as 0.892 when sperm is unity.

When the two oils were burned with a solid wick and natural flow the consumptions were, respectively, 1.675 grains, and 1.247 grains per minute at the end of the same period as employed here; giving a proportionate consumption for colza of 0.743, and showing that an increase of very nearly one-fifth has been made in the consumption by means of the artificial supply.

Dividing the photometric ratio by the increased ratio of consumption, we have $\left(\frac{0.63634}{0.89233}=\right)$ 0.713 as the specific illuminating ratio for colza compared with sperm. This ratio I have also called, in the former reports, the economical ratio, because it admits of a direct comparison of cheapness with price. Thus, if sperm be at $2 per gallon, colza would be its complete equivalent at $1 43. And this is so, leaving out of view the difference in specific gravity, amounting to about 3 per cent.; which is in favor of the colza, and allows an enhancement of its price.

The normal colza flame—that is, when, with an average consumption of 12.85 grains per minute, it is burning at its best—is smaller than the normal sperm flame, which is supported by the combustion of one-eighth more of material. But, volume for volume, the colza flame is to the eye the more brilliant, and I do not doubt, photometrically, the more illuminative.

It also, however, requires more management. The limit between its purest white and the faint tinge at its apex, soon deepening into red, which is the index of incomplete combustion and the precursor of unmistakable smoke, is much sooner passed than in the case of sperm; which last, indeed, allows a considerable margin in the setting of the wick and the adjustment of the chimney. But when the proper points are once attained, the constancy of the colza flame is not less assured than that of sperm.

I am not aware of the price at which colza oil is to be obtained. But, upon the observations herein given, and the fair deductions to be made from them, there is no doubt that colza may be advantageously substituted for sperm in the light-house service whenever the cost of the former does not transcend seven-tenths of that of the latter.

Nor do I know if colza is obtainable in large quantities at any price, or at all. If it is, and you find reason, upon what has been said, as well as upon its reputed successful employment in France, to which I have had no reference whatever as yet, to encourage its introduction, I take the liberty of suggesting that this may be conveniently done by alternating its use with that of sperm at the Lazaretto light-house, in the harbor of this city. In this resort the question admits of being solved, after a short time, in the fullest and most practical way; and I shall be at your service, whenever desired, to give personal attention to the adjustment and management of the lights, respectively; their proper photometric comparison, and all other points that are necessary to be ascertained in order to come to a sound conclusion.

I have the honor of remaining, sir, your obedient servant,

J. H. ALEXANDER.

REPORT.

The oils herein reported on were all transmitted directly from the Light-house Board, and were labelled, numbered, and received, as follows:

No. 1. U. S. winter sperm from lakes, 1855; rec'd 26th Nov.
No. 2. Sperm from Sankaty Head L. H., Oct., 1855; "
No. 3. U. S. contract, summer oil, 1855; "
No. 4. U. S. contract, winter oil; "
No. 5. Plumb I. L. H.; delivered in spring of 1854; "
No. 6. Signal oil from Buffalo, N. Y.; "
No. 7. Sperm oil from E. M. Robinson, N. Bedford; rec'd 15th Dec.
No. 8. Oil from H. S. K.; "

Examination has been had of the material characteristics of each, in the same manner as more fully detailed in former reports upon *other samples,* and with the following results.

PHYSICAL PROPERTIES OR EXTERNAL CHARACTERS.

1. *Color.*—Except in the samples No. 6 and No. 8, there is nothing distinctive in this respect. Arranged in order of increasing depth of tint, they all stand as follows:

No. 8. Very pale, like lard oil.
No. 6. Pale gold.
No. 7. Golden.
No. 5. +
No. 1. +
No. 4. Deeper gold than No. 1.
No. 3. +
No. 2. Deepest of all.

2. *Odor.*—In this respect, also, there is nothing to attract observation, except with the samples No. 6 and No. 8. The first, No. 6, has a decidedly lard-like odor, with a faint after-smell of fish-oil. The other, No. 8, emits a strong, peculiar, and persisting fragrance like that of benzole, which has been probably disguised, partially, by the addition of some aromatic essential oil, most likely peppermint.

3. *Density.*—The temperature of the chamber in which the observations for this item were made, and where the samples had been left to stand in their proper beakers for several hours, was at the beginning of the weighings 50° F., and 51° F. at their close. The resulting densities were as follows:

Water	1.—	at 50°.5 F. mean.
No. 1	0.88605	"
No. 2	0.88660	"
No. 3	0.88475	"
No. 4	0.88660	"
No. 5	0.88512	"
No. 6	0.91940	"
No. 7	0.88512	"
No. 8	0.89679	"
Robinson sperm $_{34}$ of first report	0.88420	reduced to 50°.5 F. from 64°.5 F.

In these weighings the immersion of the wire was so nearly constant that no correction has been made for this item in the calculations. It may be safely neglected. Reduction to a standard temperature of 62° F. would affect the given densities in the sense of diminution and in the third decimal place only. With No. 6 and No. 8 such reduction would be, of course, the greatest. With the other samples it will be nearly constant, and equal to about —0.004.

No. 5, which is an oil easily congealed, gave evidence of this in a slight deposit of crystals. Its specific gravity, then, determined in this state, is somewhat too low, but not sufficiently abnormal, as it appeared, to require a repetition of the observations for it at a higher and more favorable temperature.

The specific gravity heretofore obtained for the Robinson sperm$_{34}$, which may be justly looked upon as a standard oil, has been reduced to the temperature of these observations, and entered, as matter of interesting comparison, in the table. It serves to show that all the samples, except No. 6 and No. 8, are quite satisfactorily constituted in this item of density. The basis of both these Nos. appears so far to be lard-oil.

4. *Congelation and solidification.*—Experiment was made on these characteristics, both in the artificial method and with the precautions formerly described; and also in what may be termed the natural method, by exposing the several samples, in flasks, to the external atmosphere, whose extremely low temperature at one time, and for some days, was favorable for such observations. The following statement exhibits the results in this particular:

Temp.	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.	No. 7.	No. 8.
° 46					Solid'ed almost throughout.			
44						Milky ring.		
42			Ring forming					
41			Cloud'g at B.					
38		Ring.					Ring.	
37				Ring.				
36.5							Clouded.	
35	Ring.		Gelatinous.				Gelatinous	Ring.
33							Solidifying	
32	Clouding at B.							
31	Solidified at B.		Solidified.	Cloud'd at B				
30				Gelatinous.			Solid.	
28		Clouding at B.						
26		Solidified at T.		Solid.				
24.5						Cloud'g at B.		
23						Cloud'd at B.		
20.5						Gelatinous.		
17						Solid.		
14								Clouded.
11								Solidified.

The artificial temperatures were not carried below 20°.5. No. 5 sample, which was immersed in a bath of about 23°, being itself at a temperature of about 62°, was solidified so promptly as to give no opportunity for observing its preceding stages. As it was no doubt a summer oil, an old delivery, and from a stock probably by this time exhausted, it was not supposed of interest to repeat the observation.

5. *Consumption of oil in combustion.*—As in former instances under this item, the lamps were filled, weighed, lit, allowed to burn for a certain interval, extinguished simultaneously, reweighed, and so on. In this instance, the last period extended to their final spontaneous extinction, the epoch of which was different for the most of the samples. The comparison of the times with the weights, respectively, gives the rate of consumption; which is expressed in grains per minute, as in the following table:

Sample.	First period.		Second period.		Third period.		Average consumption.	Ratio.
	Time.	Consumption.	Time.	Consumption.	Time.	Consumption.		
	m.	*Grs. per min.*	*m.*	*Grs. per min.*	*m.*	*Grs. per min.*		
No. 1	185	1.6930	315	1.6645	935	1.3716	1.5764	0.98
No. 2	185	1.4190	315	1.4039	905	1.1588	1.3372	0.83
No. 3	185	1.5386	315	1.5563	995	1.2666	1.4538	0.90
No. 4	185	1.5274	315	1.4997	905	1.1220	1.3830	0.86
No. 5	185	1.6051	315	1.5720	785	1.2757	1.4843	0.92
No. 6	185	1.6769	315	1.6602	635	1.3101	1.5491	0.96
No. 7	185	1.6639	315	1.6517	935	1.2079	1.5078	0.94
No. 8	185	1.4172	315	1.3801	1145	1.0846	1.2940	0.80
Robinson standard	125	1.7998	320	1.7814	905	1.2453	1.6088	1.00

The last line refers to the observations formerly had in this particular upon the sperm $_{34}$ furnished by Mr. Robinson, and to be found in the first report. The rate of burning of this oil was so uniform for a considerable period that the factor, deduced from a period of 125 minutes, may be safely applied to a period of 185 minutes; while, as between periods of 315 and 320 minutes, the probable variation and consequent error will be still less. Upon these two, then, and upon a third term observed and given in the former report, the series has been extended, by interpolation, to a term corresponding to 906¼ minutes, which evidently may be used for 905 minutes, the actual mean of the times belonging to the third period of the table. And the average consumption for this oil, thus obtained from the observed and interpolated terms, has been used to establish the ratio given in the last column. This ratio, if not numerically exact, at least serves usefully in the way of illustration.

Connected with the rate and duration of combustion is the quantity of oil left unconsumed in the lamp at its spontaneous extinction. The particulars of interest in this respect are assembled, as observed in the following table :

Sample.	Weight of—		Proportion unconsumed.
	Initial oil.	Residual oil.	
	oz.	oz.	
No. 1	2.7947	0.1229	4.40 per cent.
No. 2	2.7297	0.5449	19.96 "
No. 3	2.7652	0.1397	5.05 "
No. 4	2.7075	0.5931	21.87 "
No. 5	2.6971	0.6108	22.65 "
No. 6	2.8235	1.0904	38.62 "
No. 7	2.7225	0.3697	13.60 "
No. 8	2.7752	0.1879	6.91 "

To ascertain how much oil remained in the body of the lamp, and how much was absorbed by and retained in the wick, experiment was made upon four of the samples, among those which had the least and those which showed the most residuum. A wick of the size employed—30 threads 5 inches long—was weighed clean, and then the four greasy wicks weighed as they were, separately. The smallest quantity absorbed by the wick was in the case of No. 7, and amounted to 0.0943 oz. troy; the largest in the case of No. 8, amounting to 0.1283 oz. The mean of all four was 0.1060 oz.

Comparing this with the last table, it will be seen that samples No. 1 and No. 3 left hardly any free oil unconsumed, while No. 6 was hardly more than one-half burnt away. These differences, no doubt, arise from normal variations in the respective fluidities of the samples and in the carbonization of wick which they severally superinduce, and also from accidental disparities in the wicks themselves. Such disparities hardly, if at all, allow themselves to be detected by any system of observation; but they cannot, at any time, with reasonable precautions, be considerable. The other elements—fluidity and carbonization—have already been the subjects of investigation in the two previous reports, and the method shown by which they are to be, at least approximately, deduced from the observations.

These methods would be again exemplified here were it not that the samples which exhibit the most remarkable differences in these respects, and would, therefore, be of the most interest to be applied as tests of the methods, are found in other important practical relations—such as, for example, their photometric ratios—to be so far inferior as to render the investigation of causes leading to that inferiority a matter comparatively of indifference. We may, then, pass on at once to the

6. *Illuminating power* of the several samples. For this, the mean of three sets of photometric observations, made at successive epochs of an hour and a half, three and five and a quarter hours after lighting, has been taken to give the value of the respective lamp-flames in terms of a standard candle. And the results obtained for the first report from the Robinson sperm, whose epochs agree nearly with these last, have been added for more satisfactory and complete illustration.

Sample.	Photometric values.		Oil consumed.	
	Lamp, in candle.	Ratio.	Grains per minute.	Ratio.
No. 1	0.7307	0.9743	1.6645	0.9511
No. 2	0.5980	0.7973	1.4039	0.8022
No. 3	0.6467	0.8623	1.5563	0.8893
No. 4	0.6480	0.8640	1.4997	0.8570
No. 5	0.6618	0.8824	1.5720	0 8983
No. 6	0.5814	0.7752	1.6602	0.9487
No. 7	0.7171	0.9561	1.6517	0.9438
No. 8	0.5365	0.7153	1.3801	0.7886
Robinson standard, 1st report, say	0.75	1. —	1 75	1. —

The first column of this table contains the samples ; the second, the experimental value of the respective lamp-flames; while the third gives their proportionate value compared with the Robinson standard. The last two columns of actual and rational consumption have been added as the materials for arriving at an economical value of the several samples.

This economical value reposes on the assumption that with the same oil there is a constant ratio between the illumination and the quantity consumed in a given time ; if such quantity be doubled, for instance, the illumination or direct photometric value will be double also, and so on. Numerically, it will be then directly as the experimental photometric values, and inversely as the grains consumed per minute ; and the economical value or specific illuminative power, as it was termed in the first report, is the experimental photometric value reduced to what it would have been had the consumption per minute been the same for all the samples.

The determination of the specific illuminative power is of practical utility when, as between several samples, the direct photometric values and quantities consumed in a given time do not differ by any large amounts. In such cases, the advantage is sometimes demonstrable in preferring a cheaper, though photometrically inferior, oil, whose luminiferous deficiency may be remedied by management of the lamp or special contrivance for increasing its normal rate of consumption. And a similar determination is also applicable in cases, like some of the present, where the differences referred to are considerable, for making a still more palpable contrast. Thus, in the last table, the direct photometric value of No. 6 does not differ very materially from that of No. 2, yet the economical values will come out quite far apart; for, in a given time, one sample is consumed more, by one-sixth part, than the other. This will be seen in the following table :

Samples.	Economical ratio.
No. 1	1.0254
No. 2	0.9940
No. 3	0.9696
No. 4	1.0082
No. 5	0.9823
No. 6	0.8172
No. 7	1.0130
No. 8	0.9070
Robinson standard	1. —

The decided inferiority of samples No. 6 and No. 8 is here quite apparent. The others are not very materially below the standard sample, and some even, as No. 1, No. 7, and No. 4, are above it. That is to say, taking the instance of No. 1, if the consumption of this could be augmented 5 per cent. by any management or contrivance—and in the lamps used for these observations an addition of two threads to the wick would be adequate to this augmentation—the light given would be 2½ per cent. more than by the normal combustion of the Robinson standard flame. And if the oils were, weight for weight, at the same price, the sample No. 1 would be the cheaper. No. 2, on the contrary, would require eight threads to be added to the wick, and then its light would be one-half per cent worse than the Robinson flame. Such an addition of wick could not be made in the lamp employed for these experiments without changing its pipe; and this serves, in its measure, as an indication of the practical limits which are set by the circumstances and construction of apparatus, to a choice among oils upon considerations like these. In the Fresnel apparatus these limits are restricted by the conditions of the angles, etc., of the glass prisms, whose dimensions and position must always be with reference to the size of the flame in the common focus. Now, sample No. 6, the signal oil from Buffalo, would require an augmentation of volume in its flame amounting to nearly 25 per cent., in order to equalize its illumination with that of the Robinson sperm. If the two flames were already normally of the same volume on the mechanical lamp of the apparatus as they appear to be in the experiments with a solid wick—the difference appearing to be in a different intensity of light accompanying combustion—it need not be matter of conjecture how far the change of size in the signal oil flame would interfere with and frustrate the action of the catadioptric parts of the apparatus.

CHEMICAL CONSTITUTION OR INTERNAL PROPERTIES.

1. *Homalism.*—All the samples remained quite neutral, showing neither acid nor alkaline reaction for a period of 18 hours. At the end of that period all, except samples No. 6 and No. 8, began to manifest a slight acid reaction.

2. *Qualitative associations.*—In the several samples were found traces of various substances as follows:

No. 1 gave chlorine and soda.
No. 2 gave chlorine, potassa, and soda, *l.*
No. 3 gave chlorine, *ss*, and soda, *ss.*
No. 4 gave chlorine, *ss*, and soda, *ss.*
No. 5 gave chlorine, potassa, and soda.
No. 6 gave chlorine and potassa.
No. 7 gave chlorine, potassa, and soda.
No. 8.
Robinson's standard gave chlorine, *ss*, and potassa, *ss.*

The letters *l* and *s* here, as in the former reports, signify that the traces of the substance to whose name they are annexed, are large or slight. The reduplication of the same letters indicates a higher degree of the characteristics, respectively.

Sample No. 8 was found further to contain 0.1612, by weight, of a volatile oil, which cannot be ascertained more nearly than that it is some liquid hydro-carbon belonging to the benzole series. The volume operated on was found to have been reduced after evaporation by 0.1765 nearly, which shows a specific gravity for the matter volatilized not much less than that of the fixed mass. But this observation does not pretend to extreme precision.

3. *Chromatic tests.*—These were applied by means of sulphuric and nitric acids and caustic soda, in the same way as described in former reports. All the samples, except No. 2, No. 6, and No. 8, gave the normal reaction of pure sperm.

No. 2, by comparison, indicates a small admixture of whale oil with sperm.

No. 6, whose fishy odor, mingling with the smell of lard, has been already spoken of, emitted the former still more decidedly upon saponification. The comparison of mixtures in a chromatic scale showed that there was no whale oil present, and the sample in question corresponded with a mixture composed of 0.89 lard oil and 0.11 sperm.

No. 8 gave no indications of fish oil of either kind.

4. *Thermal tests.*—The epitherm of the several samples, or the elevation of temperature produced in each by the addition of a certain quantity of sulphuric acid, as described in former reports, was observed as follows:

Sample.	Epitherm.
	°
No. 1	79.6 F.
2	85.
3	73.4.
4	77.
5	76.3.
6	89.
7	77.3.
8	78.4.
Robinson standard,	70. F.

These tests, in the cases of the undoubtedly adulterated samples, will be found to accord very tolerably with the results of direct phoometric comparison. If we assume that the two tests are strictly roportionate, No. 2 ought to have a photometric value, calculated rom the epitherm of 0.6176 candles instead of 0.5980 candles, as observed, or *vice versa*, an epitherm of 87°.5 F. calculated, instead of 5° F. observed. In the same way No. 6 would have a calculated epitherm of 90°.5 F., instead of 89° F., and a calculated photometric value of 0.5899, instead of 0.5814 observed.

As for No. 8, which contains no fish oil, but whose adulterations are of vegetable origin and of some substance whose epitherm is yet unobserved and unknown, it cannot of course come into the category of being calculated at all.

The other samples without having been adulterated in store, as is probable, are yet shown to be oils inferior to the standard by this test as by all the others, though the indications do not run parallel. The discrepancies existing here only make it of more interest to explore thoroughly and determine the epithermal limits which belong normally to the class of pure native sperm oils.

J. H. ALEXANDER.
CAMPBELL MORFIT.

REPORT

ON

OG-SIGNALS BY STEAM:

ADDRESSED TO THE

COMMITTEE ON EXPERIMENTS

OF THE

U. S. LIGHT-HOUSE BOARD.

Professors A. D. BACHE and JOSEPH HENRY,
Committee, etc., etc., U. S. Light-house Board.

BALTIMORE, *December* 29. 1855.

GENTLEMEN: I have the honor herewith to communicate certain experimental results, and to offer some suggestions upon the subject of steam fog-signals, which you were pleased to refer to me some time ago. The observations and all that part of the investigation which depended on myself have been completed many months since; the plan of the apparatus, the estimates of cost, etc., which required the technical co-operation of others in whose art such things lie, have been delayed by circumstances over which I had no control.

The difficulties of the question did not occur so much in the want of efficiency in the agent—steam—or in the implement—the ordinary steam-whistle—proposed to be employed, as in the judiciousness of the application and economy of adaptation. The former point had been long since settled by the experience of railroads, where it had been found that sounds through such an implement were distinctly audible and characteristic over distances of six and seven miles—distances at least thrice as great as are demanded for signals in the light-house service. The latter points remained, however, for investigation.

These are supposed to be covered and satisfied by the determination of the quantity of steam at various pressures blown off through the whistle in given times; of the size of boiler necessary to supply the required quantity of steam; of the cost of construction and maintenance of such boiler and its equipment; of the fuel for and attendance upon the same; of the aptitude of the ordinary light-house keepers for such attendance; and, finally, of the risk incurred either from disaster of explosion or from deficiency of steam to answer its duty. These, then, are the principal topics treated of in what follows.

In advance of these, however, and in reference to what was just now said upon the sonorousness and space-penetrating power of the ordinary steam-whistle, the course of the observations furnish some results that may be noted. On three several days, between two stations, three and a half miles apart in a straight line—the one being at Winans' Locomotive Works where the whistle was erected, and the other at or near the Lazaretto—the whistle, blown with peculiar mots (in order to distinguish it from any other chance engine-whistle) every five minutes between 10 a. m. and 2 p. m., was tinctly audible at all pressures between 25 lbs. and 62 lbs. per square inch, and at all sets of the bell between 1.5 inch and 2 inches; the set of the bell, *i. e.*, the distance of its lower edge from the orifice whence the steam issues, of course varying inversely the pitch of the musical sound emitted.

The mode in which these observations were made was this: An intelligent and reliable assistant, whose watch had been set with my own, was stationed at the boiler, and instructed to blow the whistle every 5 minutes from the hour agreed on for starting, and to note successively the pressures in pounds above the atmosphere, read on an Ashcroft steam-gauge which had been adjusted to the boiler, corresponding to the epochs of time, respectively. I myself went meanwhile to the Lazaretto station, and there noted the time epochs and the audibility of the signal and other circumstances, such as the wind, etc., tending to modify such audibility. The actual pressure at the several epochs, which of course was unknown to me during the observations, was afterwards supplied from the memorandum of the assistant. I was glad to find, upon thus filling up the table of observations, that my recorded impression of the intensity of the sounds coincided directly with the scale of pressure. On one of these days, in particular, the wind was very strong in gusts, blowing at an angle of about 50° with the line joining the two stations. I had no means of measuring the force of this wind, and still less of measuring the intensity of sound; but I find upon the journal cases of less audible sound under a stronger wind coupled with differences of pressure of steam of 5, 6, and 7 pounds. The perturbation then, and loss of effect caused by the different force of the wind, was at least equivalent to the recorded differences in steam-pressure. This loss of effect became more discernible still, when, in addition to change of force, there was also, as in some instances, a change of direction in the wind. A difference of this latter sort, amounting to say 10° or 12°, corresponds in one case to not less than 10 lbs. of steam-pressure.

These approximate evaluations serve to show that the influence of this circumstance—the strength and direction of the wind—is quite important in estimating the effect to be expected from a given fog-signal. And the point would have been further and more particularly studied, had I not understood that the distance between my two stations was excessive, in fact more than double of what would be *demanded of fog-signals* in the light-house service. If this is not the case, then it would be interesting and useful to pursue the research in this aspect more thoroughly.

It is also to be stated that, in these experiments, the whistle was adjusted at a considerable height—say 30 feet—above the top of the boiler. This position was necessary in order to carry it outside and clear of the ridge-pole of the roof. As it was, there were still buildings of greater height intervening on the line between the two stations; a disadvantage to which a regular fog-signal will not be exposed. But, however this may be, the length of pipe through which the steam had to pass from the boiler where its pressure was measured to the place of exit where its pressure was efficient, contributed to its cooling and to a consequent loss of elasticity. The recorded pressures, then, are all somewhat too high. How much too high I am not able to say with any precision; for I did not then see, as I think I do now, how a reliable steam-gauge could be suitably applied at the orifice of the whistle. An approximation, however, to the probable loss of pressure in the steam-pipe may be obtained by the following considerations:

From Tredgold's experiments* it appears that the effect of radiation from iron pipes of the same character of surface with those used in our case may be expressed by the following equation:

$$0.000738\ s.\ (T - t) = n,$$

in which s is surface in square feet of cooling cylinder; T is assumed temperature of such surface; t is assumed temperature of surrounding medium, and n is number of degrees Fahr. to which one cubic foot of water will be heated in one minute by radiation from and contact with such cooling surface.

In the case in question, an inch pipe 30 feet long has a surface of 7.854 square feet; the temperature of the enclosed steam at 40 lbs. above atmospheric pressure is 289° F., which may be taken as the equivalent to the temperature of the surface itself, and a fair average of all the pressures observed, while the external temperature during the experiments was pretty steadily about 79° F. Also, as the cooling of the radiating mass and the heating of the absorbing one are absolutely the same, and the expressions for both correlative with their respective specific heats, and as the specific heat of steam at the temperature and density here taken is nearly 0.0007, that of water being 1, at the same volume we have $n' = \frac{n}{0.0007}$ = the number of degrees Fahrenheit to which one cubic foot of steam will be heated, if that be considered the standard of effect instead of water, under the same circumstances.

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* *The Principles of Warming and Ventilating*, p. 55: Lond. 1836; 8vo.

Substituting these quantities we have the numerical equation—

$0.00074 \,.\, 7.85 \,.\, 210^\circ = 0.0007\, n'$, and finally $1714.7^\circ = n'$

for the number of degrees that one cubic foot of steam will be cooled in one minute under the given conditions. This cooling would be equivalent to the entire condensation of more than one cubic foot; but during the period of one minute the quantity discharged through the whistle has been 225 cubic feet; so that the effect upon the whole mass of steam during the given interval, supposing that mass to be presented all at once, isolated and left to cool gradually, will be $\frac{1714^\circ.7}{225} = 7^\circ.6$ nearly in reduction of temperature. This reduction from 289° F. to 281°.4 F. corresponds to a loss of pressure of very nearly 6 pounds,* so that the recorded pressure of 40 pounds above the atmosphere becomes probably in fact but 34 pounds. This reduction will, of course, be greater for the higher pressures and less for the lower ones; but it is not necessary to calculate for any but the average.

It appears, then, from the experiments that the steam-whistle, at its ordinary set, utters sounds which are audible at a distance of 3.5 miles, in a calm atmosphere, under an actual pressure of 20 pounds and upwards; that these sounds are sufficiently distinct, even when thwarted by a strong wind, at a pressure of 40 pounds; and that a competency of the boiler to work at a pressure of 75 pounds will probably render it effective under the most unfavorable circumstances. All these pressures are rated above that of the atmosphere.

In endeavoring to make a scale of audibility and distinctness, under these various conditions, other than the momentary impressions on the individual auditory nerve, which is liable to capricious alterations of sensibility, resort was had to an indication that was of interest also in other respects. This was the precision with which the local source and direction of the sound could be assigned. Uniformly the fainter the sound, the more vague is its presumed direction, while in proportion as its intensity increases, it can be ascribed within small and smaller limits to a particular quarter. In applying this test, the best mode I could devise was to close my eyes just before the moment when the sound was expected, to turn round two or three times in order to confuse and lose as much as possible any conscious orientation, and then when I heard the sound to point, still with eyes closed, in the direction along which it seemed to reach me. As the station itself was visible from where I stood, the deviation of the supposed from the true line was readily observable.

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* J. H. A. on the Pressure of Steam, etc. Sill. Journ. 2d ser. 362.

As an additional precaution to prevent the mere mechanical repetition of a previous impression, the observer's station was not a single point, but was varied in a range of three or four hundred years. When the pressure was 40 pounds and upwards, the direction of the whistle could always be determined within an estimated arc of three or four degrees. Between nearer stations there is no doubt of ability to determine the direction within a much smaller arc ; and upon the whole it may be said that in respect to this essential characteristic of a reliable fog-signal, the precision with which its site can be assigned from its sound, the arrangements as experimented on seem to be sufficient for the purpose.

In connexion with this characteristic, observations were made upon the effect, from altering the tone of the whistle or the pitch and timbre, of the sound emitted. This is done by raising or lowering the bell above the orifice whence the steam issues; and it will be better understood by reference to the accompanying drawing in section of the steam-whistle, and to the explanations and description which follow.

On the drawing, A B shows an iron stem tapped at the end A, with a long screw-thread, along which travels, in being elevated or depressed, the bell or inverted goblet C D E, of brass, whose lip C E is worked down to as fine a feather-edge as possible. The other end, B, of the stem is enlarged so as to leave room for turning out the cylindrical chamber N, one half length of the walls of which are likewise screw-tapped, in order to enter and hold steam-tight the erect hemispherical cup H K L M. The screw-thread is continued in the shank of the cup in order to couple with the exhaust steam pipe from the boiler. The walls of the chamber N, above the screw, are pierced with a suitable number of openings, O O, which allow the passage of the steam from the said exhaust into the body of the cup. The quasi lid of the cup, Q Q, of brass is attached to and rests on a shoulder of the stem B, and is separated from the lip H M, by a space of about one fiftieth of an inch, thus leaving an annular opening of that width all round the lip. The principal pains to be taken are in the central adjustment of the cup and lid, so that the opening may be of the same width everywhere, and in the adjustment of the bell in such wise as that the feather-edge before mentioned shall everywhere bisect, in its horizontal projection, the aforesaid annular opening.

The operation of the implement is now very manifest. The steam, when the exhaust-cock into the boiler is opened, rushes up through the exhaust-pipe into the chamber N, thence through the orifices O O, into the body of the cup, whence it issues through the annular opening, impinging upon the feather-edge of the bell and setting the whole metallic mass in vibration, thus producing, according to the rapidity of those vibrations, the musical sound in question.

The vibrations appear to depend both on the actual pressure of the steam at its places of exit and impact, (in which expression the vertical height between the lip of the cup and that of the bell is involved,) and on the capacity of the bell itself. No doubt they depend also upon the circumscribing mass of the bell; but in what relation, experiment has not yet given the means of judging. The most obvious and easy way of affecting the vibration, and thus modifying the tone of the instrument, is by doing what has been already spoken of, but can now be better understood on changing the set of the bell, *i. e.*, by means of the screw-thread at A, and the tightening nut D, adjusting the feather-edge nearer to or further from the annular opening.

In the sketch, the bell is represented as screwed up to its highest point, but observations were made with it at regularly successive heights from a quarter of an inch above the cup, *i. e.*, the distance C H or E M brought down to 0.25 inch, onwards. A summary of these observations is presented in the following table:

Steam pressure.		Height of bell.	No. of observations made.		Remarks.
Range.	Mean.		Distinct.	Faint.	
lbs. *lbs.*	*lbs.*	*in.*			
…………	35	0.25	…………	…………	No sound.
…………	35	0.50	…………	…………	Hardly heard.
20 — 51	38	0.75	…………	4	Very faint.
24 — 62	41	1.00	…………	10	
43 — 58	53	1.25	4	1	
23 — 40	31	1.50	5	1	Tone sensibly more grave.
30 — 54	46	1.50	6	…………	Wind stronger.
21 — 52	34	1.75	4	3	Tone more grave.
21 — 49	33	1.75	2	3	Wind stronger.
25 — 57	41	2.00	9	3	Tone not apparently altered.
40 — 58	51	2.25	4	…………	Tone sensibly more grave.

The fourth and fifth columns are introduced here merely as an approximate indication of the effect on the intensity of the sound from the prosthema or augmented height of the bell which is registered in the third. This indication here is only accidental, and gathered from a series of experiments not instituted in this special view, which was purposely deferred. In order to derive a precise result from such special experiments, it will be of course necessary to maintain an identity of pressure throughout, and to arrange a second whistle with its bell at a constant height by the side of the other so as to obtain a standard for simultaneous comparison. In this way we can determine the effect of a given prosthema in modifying the quality, *i. e.*, the gravity or acuteness, of the tone, and also the relative intensity or sonorousness of different tones of the same bell; and if with this we couple a systematic change of pressures, and apply some suitable measure of the actual force of the wind at the corresponding epochs, it is possible to obtain the equivalent of either one of these elements in terms or in some function of the other, and thus anticipate and provide for the solution of a problem of no small practical importance in actual fog-signals.

As it is, however, the table given serves to show—what also agreed with my impressions at the time and before I had an opportunity of grouping and comparing the observations—that a prosthema of about 1.5-inch affords the maximum of sonorousness for a bell of the dimensions in question. And this likewise agrees nearly with the general opinion and practice—founded, I suppose, upon experience even if unsystematic—of those who are concerned in the use of these implements upon railroads.

But among these persons so concerned, I could find no opinion founded upon experience or otherwise as to the quantity of steam necessary to be supplied for working the implement in use; and this, therefore, one of the points mentioned above as essential to be known, had to be made matter of specific determination. The description of the methods used in making this determination, I shall present here as succinctly as possible; and offer it at all only because you ought to have an opportunity of judging for yourselves of the appropriateness and reliability of the several steps that have been taken, and of the soundness of the considerations upon which those steps were grounded.

The boiler used for the purpose was originally for a locomotive engine; but had been converted into a stationary one for driving machinery at the works of Mr. Winans, in this city, by whom it was placed at my disposal. The description of it and of its appliances may be materially abridged by exhibiting the accompanying longitudinal section, drawn to a scale of $\frac{1}{48}$ of the actual dimensions.

The water-gauge was a glass tube of half-inch bore, quite uniform, and divided by a scale of equal parts, each inch, numbered successively downwards, and fitted into steam-tight caps with shut-off cocks in branches with flanges to allow of being screwed on upon the face of the boiler, as shown. The boiler was then filled with water up to the level, A, of the dome; the shut-off cocks being open both above and below. And directly afterwards the water was drawn off by the ordinary discharging cocks into a vessel of known capacity, viz: 0.633 cubic feet. From the level at which the water-gauge was applied it is evident that a good deal of water, all that contained in the dome and upper part of the cylindrical segment, would be discharged before the water surface in the boiler could be indicated by any of the divisions of the tube. In point of fact, the 20th bucket full showed the level of that surface at 0.10 of the scale and so onward, as is seen in the following extract from the notes made at the time·

Buckets drawn.	Divisions of scale.	Corresponding capacity of boiler.
		Cubic feet.
19	....	12 03
20	0.10	12.65
21	1.10	13.29
22	2.	13.93
23	2.875	14.56
24	3.625	15.19
25	4.50	15.83
etc.	etc.	etc.

As this extract is only given to illustrate the mode of observing, it is unnecessary to continue it through the 41 divisions observed, the last of which happened to coincide exactly with the 77th bucket drawn off, and had already descended further below the average water-line than it would be safe in practice to allow.

These irregular fractional divisions of scale can be readily converted into a unitary progression of whole numbers by a suitable interpolation applicable also in the last column, and yielding there a new series of values for capacity of boiler or steam-room. This is shown in the following table of—

Value of divisions of water-gauge in cubic feet of steam-room.

Division.	Steam-room.	Division.	Steam-room.
0	**12.59**	13	**22.58**
	0.63		0.84
1	**13.22**	14	**23.42**
	0.71		0.85
2	**13.93**	15	**24.27**
	0.72		0.84
3	**14.65**	16	**25.11**
	0.81		0.84
4	**15.46**	17	**25.95**
	0.73		0.85
5	**16.19**	18	**26.80**
	0.72		0.84
6	**16.91**	19	**27.64**
	0.82		0.85
7	**17.73**	20	**28.49**
	0.72		0.84
8	**18.45**	21	**29.33**
	0.81		0.84
9	**19.26**	22	**30.17**
	0.79		0.85
10	**20.05**	23	**31.02**
	0.84		0.84
11	**20.89**	24	**31.86**
	0.84		0.85
12	**21.73**	25	**32.71**
	0.85		

This table was in the original preparatory calculation extended through the whole range of the scale; but the divisions inscribed here are all that were actually employed, and all that need be given. The ciphers in small type are the first differences of the quantities of steam-room; and their occasional irregularity is to be attributed, I presume, to the existence of certain stay-bars inside of the boiler, which lie within the range of level where these numerical irregularities occur.

Having now the means of determining the steam-room in the boiler corresponding to any stand of water-gauge, it is easy, provided the observations can be made accurately enough, to determine the quantity of steam made between any two successive epochs of time and of water-gauge. For this we need, besides, only the observation of the pressure or the temperature of the steam during the interval in question. If the volume of water evaporated during said interval be taken as unity, the volume of steam made will be represented nearly enough for practical purposes by this equation:

$$V_{,} = \frac{Vp}{p'} \cdot (1 + 0.002.\ \overline{t' - t.});$$

in which V = volume of steam at 212° F., a temperature taken as equivalent to the ordinary pressure of the atmosphere, and = 1695 when the volume of water at that temperature yielding it = 1.

p = atmospheric pressure = 15 lbs. per square inch.

p' = actual pressure observed in pounds per square inch, and if taken with the usual steam-gauge to be augmented by 15 lbs.

$t_{,}$ = temperature corresponding to p'; and

t = 212° Fahr.

$Vp = 25425$.

From this equation was constructed, for greater convenience, the following table, which, as it may hereafter serve in connexion with the prosecution of the subject, and is not readily to be met with elsewhere, is here presented, calculated through as great a range of pressures as are at all likely to be required for fog-signals:

Table of the pressure, temperature, and volume of steam; water evaporated being 1.

Pressure.	Temperature.	Volume.	Pressure.	Temperature.	Volume.	Pressure.	Temperature.	Volume.
Pounds.	° *Fah.*		*Pounds.*	° *Fah.*		*Pounds.*	° *Fah.*	
15	212	1695	35	260.3	796.6	55	288.9	533.4
16	215.6	1600.4	36	262	776.9	56	290	524.9
17	219	1515.3	37	263.6	758.1	57	291.2	516.6
18	222	1440.8	38	265.3	740.3	58	292.4	508.9
19	225	1373	39	267	723.6	59	293.5	501.3
20	227.8	1316.5	40	268.5	707.4	60	294.6	493.8
21	230.5	1255.5	41	270	692	61	295.7	486.6
22	233	1208.9	42	271.5	677.3	62	296.8	479.7
23	235.5	1157.4	43	273	663.4	63	297.9	472.9
24	238	1118.8	44	274.5	650	64	299	466.4
25	240.4	1074.7	45	275.9	637.2	65	300	460
26	242.7	1037.9	46	277.3	624.9	66	301	453.8
27	244.8	1003.4	47	278.7	613.2	67	302	447.8
28	247	971.6	48	280	601.7	68	303	442
29	249	941.6	49	281.3	590.9	69	304	436.3
30	251	913.6	50	282.6	580.3	70	305	430.8
31	253	887.5	51	283.9	570.2	71	306	425.4
32	254.9	862.7	52	285.2	560.6	72	307	420.2
33	256.7	839.4	53	286.5	551.2	73	307.9	415.1
34	258.5	817.3	54	287.7	542.1	74	308.8	410.2

The next step was the attempt to measure the quantity of water evaporated during given intervals. For this the fire was got up in the boiler; and when the pressure of steam was well up, and increasing uniformly, the stand of the water and steam-gauges was read, the time noted, and the exhaust-pipe leading to the steam-*whistle opened* at the moment. After a certain interval, the steam still blowing off through the whistle, the gauges were again read; and so on through the whole series. Of these observations, an example is given in the following extract from the note-book:

Time.	Water gauge.	Pressure.	Steam room.	Temperature.	Volume of steam, water = 1.
h. m.	°	*Pounds.*	*Cubic feet.*	°	
11 26	16.5	40	25.53	288.9	533.4
11 30	18	31	26.80	277.3	624.9
11 40	18.5	33	27.22	280	601.7
11 45	20.5	34	28 91	281.3	590.9
12 10	21.5	45	29.75	294.6	493 8
12 15	24	50	31.86	300	460

Of these, the numbers in the first three columns alone are from direct observations; those in the last three are tabular numbers from the preceding tables. The intervals in the series, of which these form a part, were intended to be from 5 min. to 5 minutes; but it was found necessary several times to shut off the exhaust, etc., so that these observations are not regularly consecutive, but have to be taken in pairs. To illustrate the mode in which they have have been applied, it will be enough to take the first pair.

The difference in the stand of the water-gauge for these, viz: 1°.5 corresponds to an evaporation of water or augmentation of steam room, as shown in column 4, to amount of 1.27 cubic feet in the interval of 4 minutes. And if the initial pressure of 40 pounds above the atmosphere had been sensibly maintained through the whole interval, the boiler would have been making (1.27 × 533.4 =) 677.4 cubic feet of steam, of temperature 288°.9 F., in that time. Also, the maintenance of such pressure would indicate that the exhaust was working off just the quantity supplied, and neither more nor less, while an increase of pressure would show that the supply was gaining upon the consumption, and that an accumulation of steam was taking place, and *vice versa.* In fact, however, we see, from the second observation, that the pressure fell during the interval, and that the initial and residual densities of the steam were in the ratio of $\frac{1}{533 \cdot 4}$ to $\frac{1}{624 \cdot 9}$; which change of density is both a consequence and a measure of the consumption. The steam made and steam consumed are inversely proportionate to the observed densities at the beginning and end of a given period, or, what amounts to the same, *the steam made is* equal to the number of cubic feet of water evaporated during the period in question, multiplied by the volume or tabular factor of expansion corresponding to the initial pressure, while the steam consumed is equal to the same number multiplied by the expansion due to the residual pressure. Thus, for the case in question, where the boiler was making 677.4 cubic feet at 289° F., the consumption was (1.27 × 624.9 =) 793.6 cubic feet, at 289° F., in 4 minutes, or 198.4 cubic feet per minute, at a pressure of 40 pounds above the atmosphere. Treated in the same manner, the second pair of observations give (1.69 × 590.9 =) 998.5 cubic feet, at 280°, in 5 minutes, or 199.7 cubic feet per minute, at 33 pounds above the atmosphere; and the third pair, (2.11 × 460 =) 970.6 cubic feet, at 294°.6, in 5 minutes, or 194.12 cubic feet per minute, at 45 pounds.

Yet, accordant as these results appear to be, I could not but regard their agreement as in good measure fortunate. The device of the method was well enough, and the appliances seemed to leave little or nothing to desire; and while the boiler contained only cold water, the water-gauge indicated the level within very precisely. But when it contained steam, the vapor in the upper part of the gauge condensed irregularly, and, falling down in drops, materially disturbed the accuracy of reading; so that, after a dozen and more observations, the mean of which, without regard to temperature, gave me a consumption of 219.6 cubic feet per minute, I resolved to resort to another method.

This was to measure the annular orifice through which the steam issued. It was impossible, in the nature of the case, to make this measurement directly with any reliability. But I suppose it could be done indirectly with sufficient precision by affixing the whistle, provided with a suitable stop-valve in the chamber N, horizontally to a water cistern of known dimensions; allowing the water to be discharged through the annular orifice during certain periods; measuring the head of water above the centre of the orifice at the commencent and end of those periods; ascertaining thus the whole quantity of water discharged, as well as the velocity of efflux; and with these elements calculating back to the area of orifice properly corresponding.

In this view, then, upon a cistern having a rectangular surface of 3.7664 square feet, and suitably fitted, were made the following observations of particulars, and were obtained the accompanying final results:

Period.	Head.		Depth of the prism discharged.	Results.	
	Initial.	Residual.		Mean velocity in feet per second.	Area of annular orifice in square feet.
Section.	*Inches.*	*Inches.*	*Inches.*		
60	15	12.85	2 15	5.38211	0.0020877
60	15	12.90	2.10	5.38714	0.0020392
60	15	12.75	2.25	5.37204	0.0021442
60	15	12.80	2.20	5.37709	0.0020955
60	15	12.75	2.25	5.37204	0.0021442
61	15	12.70	2.30	5.36699	0.0021287
60	15	12.85	2.15	5.38211	0.0020469
60	15	12.80	2.20	5.37709	0.0020955
60	15	12.85	2.15	5.38211	0.0020469
60	15	12.80	2.20	5.37709	0.0020955

'In making the calculation of the last two columns, it was thought proper, in every case except the first two, to allow an additive correction of 1.25 seconds to the period inscribed, in order to cover the inaccuracy that would have occurred otherwise from the mode of discharge, which was not simultaneous at the moment of beginning over the whole orifice; but, commencing first at the lowest point of the circumference, extended itself promptly, but progressively, upwards, until it embraced the whole area. This is mentioned to account for the apparent discrepancy between the area given by the 1st and that by the 8th and 10th observations.

The arithmetical mean of all the areas is 0.0020924 square feet, which may be represented nearly enough by 0.0021 square feet taken n the subsequent calculations, as the precise area of the annular opening sought.

These subsequent calculations were in applying the dimensions which had thus been obtained to the case of steam issuing through the annular orifice, and were made according to the following formula, viz: $Q = s \, . \, \sqrt{2 \, g \, H \, . \, \frac{\Delta}{\delta}}$ = quantity discharged per second.

In this, s = area of orifice in square feet.

g = gravity, taken at 32 feet per second.

H = height in feet of a column of water equilibrating the pressure of steam in any case.

Δ = density of the water (or any other fluid) assumed for the manometric column; and

δ = density corresponding of steam.

I took here water as the manometric fluid, reckoning a column 33⅓ feet high as equivalent to a pressure of 15 pounds or 1 atmosphere; because of the convenience of using the tabular factors of expansion given on page 108 ante. Using these factors, δ becomes equal to unity throughout; and the fraction $\frac{\Delta}{\delta}$ equal to the appropriate factor itself.

Substituting the constant numbers in the equation, clearing them as far as possible of the radical, and observing that $60\ Q = Q' =$ quantity discharged per minute, we have $Q' = 60 \cdot 8 \cdot .0021.\ \sqrt{H\Delta}$ $=$ cubic feet per minute, $\therefore Q' = 1.0080\ \sqrt{H\Delta}$, and finally, near enough for practical purposes, $Q' = \overline{H\Delta}^{\frac{1}{2}} =$ cubic feet of steam per minute. Applying this to an average case of 40lb. pressure above the atmosphere, we have $H = 2\frac{2}{3}$ atmosphere $\times\ 33\frac{1}{3}$ feet $= 88\frac{8}{9}$ and the tabular value of $\Delta = 533.4$ corresponding to 55lb. pressure, and consequently $Q' = (\frac{800}{9} \cdot 533.4)^{\frac{1}{2}} = 217.75$ cubic feet. The direct estimation by the water-gauge (p. 109 ante) gave 198.4 cubic feet.

If we take the extremest case that is likely to occur, and the utmost to which the boiler need be taxed, viz: a pressure of 75lb. above the atmosphere, we shall have a consumption of steam equal to 240 cubic feet per minute. The density of this steam, which is of temperature 322°.6 F., is very nearly $\frac{1}{345}$ that of water; and the 240 cubic feet at that temperature would require therefore the evaporation of $\frac{240}{345} = 0.7$ cubic feet of water, or say 5.25 gallons per minute. For such an evaporative power, then, the grate and fire-surface of the boiler should be proportioned.

In making these proportions and those of the other accessories, I procured the help of Messrs. Winans & Whistler, expert and successful engine builders of this city; and a general design for an upright tubular boiler was arranged, of which the sketches in the margin show a vertical section through the middle and a ground plan of base. Of course, a boiler working with the pressures of steam that have been mentioned requires to be fed by a pump, and instead of a hand pump, which at even the lower pressures would be quite laborious, and would no doubt furnish an item for complaint, it was thought best to attach machinery to be worked by extra steam. This steam pump and fixtures can all be contained within the square of the base.

Such a boiler and apparatus of the general design and external *dimensions* as given and warranted to do the prescribed work of evaporation, the parties named will formally enter into contract to furnish complete within a month from the date of the order for the sum of twelve hundred and fifty dollars.

I omitted to state in the proper place that the smoke-stack, which on the sketch is exhibited in fragment, will require to be about twenty feet in height. It might, with advantage, be even higher if the circumstances of the locality admit.

The price given is for the apparatus turned out of the shop. In addition to this cost will be the expense of transportation to the place that may be chosen for its erection, and the transport and subsistence of a workman from the shop, who would be occupied say three days in putting together these parts—pump, stack, etc.—that are better shipped separately.

I have considered the design, and besides my long established confidence in the parties, am satisfied, upon my own convictions, of its efficiency, and also of the reasonableness of the price asked; and I therefore do not hesitate to recommend, if you, upon consideration, approve of the preliminary information that has been obtained, and the conclusions which have been arrived at, that the execution of the design be undertaken accordingly.

At the same time, I should not omit another suggestion as in this connexion worthy of being considered. The boiler itself used for these experiments is now on hand, in reasonably good working order, and capable of use for a long time to come. The gratuitous use of it has been offered to me for any further experiments, at any other place, during any reasonable period. It has occurred to me that avail might be judiciously taken of this offer, either instead of the immediate construction of the new boiler, or for the purpose of some further experiments, to be spoken of presently. Either of these alternatives would promote economy, assuming that you approve of the fitness of the latter contingency. In connexion with the former, it is to be considered that the existing boiler is more bulky, and nearly twice as heavy as the proposed one will be. It would, therefore, be more troublesome to remove once, (and it will require in fact two removals, one forth and the other back,) and more expensive to shelter. The weight of these considerations, however, would be very much modified by the circumstances of the locality where it might be determined to place it.

But leaving these contingencies I pass rather to some other topics more nearly connected with the point at issue. The cost of construction of the proposed boiler has been already given ; that of its equipment and shelter will depend very much upon the judgment and taste exercised in its erection. All that is absolutely indispensable is a wooden shed to protect it from the weather. In the same shed there would be room enough for the store of fuel, if that be coal ; if it be wood, there is less occasion for protecting it. Instead of having the signals made by hand, which would be tedious and dependent upon individual accuracy and fidelity, it would be proper to have the whistle valve opened at suitable intervals by machinery attached to the pump. In this way a greater regularity would be insured, and characteristic distinctions in the mots blown could be easily established. I do not know if such distinctions are as desirable here as in the case of light signals ; there would be at least this advantage of discriminating between the fog signal and the accidental sounds from steamers, for instance, that might be in the vicinity unseen. It would not be difficult, indeed, by making the bell movable, in a vertical sense, to graduate a succession, at pleasure, of sounds in a musical scale, and thus to make the instrument, in fact, play a tune. But even if we dispensed with this refinement as a too luxurious ingenuity, there is a certain advantage in having a succession of different notes rather than a monotone, in that with the same intensity the sound of the former is more audible. The whole expense of this equipment and shelter may be rated from fifty to two hundred and fifty dollars, according to completeness and extent.

The cost of maintenance depends very much upon the care taken in management, and upon the more or less frequent use of the apparatus. As sea-water has to be used, there will be deposit and corrosion in the boiler very promptly and to a destructive extent, unless particular attention be paid to timely and frequent blowing off. With proper care, assuming that the boiler is worked for one-sixth of the year, the annual expense of repair need not transcend 5 per cent. of the cost, or say seventy dollars upon the boiler and machinery proper. I consider one hundred dollars as a large allowance.

The expenditure of fuel will be more considerable, and all the more because of the liberal allowance that ought to be made for blowing off, in order to spare the boiler. The average consumption in this particular may be set down at two and a half tons per day of *24 hours, the whistle* being blown continually. In point of fact, it would be more judicious to have intervals regularly alternating, of silence. The expenditure of fuel would also be proportionately reduced.

If there are two keepers at the light, they are sufficient for the requisite attendance, both as respects time and competency. The care of the steam apparatus demands less sedulity and not more mechanical skill in manipulation than the apparatus of a revolving Fresnel light.

There is one point in which the two concerns differ, to be sure. Carelessness about the lamp, etc., has in general no other consequence than the damage to the machinery and the diminution or extinction of the light. Carelessness about the steam apparatus may be followed by a disaster personally affecting, in limb or life, the attendants. It is probable that such a constant liability, individually, will rather increase than diminish the reliability of the keepers, and will tend to promote their assiduity.

With ordinary attention to the steam and water-gauges, and to the blow-off cock, the routine and contingencies of which can easily be made the matter of a very brief and intelligible instruction, the risk of either deficiency of steam or explosion is practically nothing. And I cannot find in the possible abuse of this agent—steam—any argument against its judicious and easily-manageable use for a purpose to which it is so eminently applicable, and for the accomplishment of which it seems indeed to furnish the sole unquestionable resource.

A few pages back I spoke of some further experiments that were desirable to be made. These do not relate to the general merits or efficiency of the system of fog-signals by steam, which I consider as established with all reasonable certainty by what has been done. They concern modifications and improvements in the apparatus as now designed, and refer principally to the effect which the form, size, mass, and set of bell have separately or together upon its sonorousness. In this connexion, too, it would be proper to try the effect of different areas of annular orifice; although from the history of the steam-whistle, and the successive steps of its development, I consider it highly probable that no useful resort will be found in lessening the annular dimensions that have been experimented on. As, however, a quite minute variation here materially affects the quantity of steam demanded, it is possible that a sufficient sonorousness and characteristic distinctness of tone may be obtained with a smaller implement.

Experiments like these would already have been tried under the authority presumed from your general instructions, except for the inconveniences of the circumstances. They would have required a removal of the boiler, which could not until very recently have been spared, or of some other, which could not be procured except upon conditions which would have been extravagant, to some place without the city. As it was, our experiments at the works were disturbed, and I confess, not without reason, by the interference of the municipal police. The disturbances, to be sure, did not amount to anything; for, in anticipation of some such consequence from the fearful noises accompanying the experiments, they were so programmatized as to allow, at various intervals, of a cessation, and thus to manifest all becoming respect to the authorities. But in the remaining series such intervals cannot with convenience and economy be allowed; and the only resort is to posit the boiler either entirely without, or at some unpopulous place within, the municipal limits.

As I have mentioned this subject now, I await, under the circumstances, your further instructions. And having thus communicated in what precedes all that is necessary (and, I begin to fear, more than is necessary) to a full understanding of the matter of report, I refer the decisions to your wise discretion.

And have the honor to remain, gentlemen, with profound respect,

J. H. ALEXANDER.

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